CAL/USAAVLABS SYMPOSIUM PROCEEDINGS

AERODYNAMIC PROBLEMS ASSOCIATED WITH V/STOL AIRCRAFT

VOLUME IV

FEATURED SPEAKERS, PANELS, AND TECHNICAL DISCUSSIONS

22-24 June 1966

Statler-Hilton Hotel

Buffalo, New York

CAL/USAAVLABS

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Published in Four Volumes as Follows:

Volume I	Propeller and Rotor Aerodynamics
Volume II	Propulsion and Interference Aerodynamics
Volume III	Aerodynamic Research on Boundary Layers
Volume IV	Panels on Recommended V/STOL Aerodynamic Research, Panel Summaries, Featured Speakers, and Technical Paper Discussions

This is VOLUME IV and contains the Major Addresses, Panel Papers and Summaries, and Technical Discussions for all Technical Sessions

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FOREWORD

The rapid advances made in helicopter and V/STOL aircraft development in the past few years have spotlighted areas in which significant aerodynamic problems have been encountered, and in some cases the problems still exist. Fortunately, a technological maturity has now been attained in the industry, making possible investigations through knowledgeable scientific approaches that have produced an enlightened understanding of the problems and, in many cases, have led to practical solutions. The next generation of flight vehicles, currently in the design and development stages, are offering challenges to the aerodynamicist and engineer, and it is evident that future vehicle developments will demand an ever-increasing rate of technological advance in the knowledge and understanding of aerodynamic phenomena.

Several years have passed since a technical specialists' meeting was held to direct attention specifically to the low-speed aerodynamic problem areas of helicopters and V/STOL vehicles. Therefore, in the interest of disseminating up-to-date information, the cosponsors of this symposium, the U.S. Army Aviation Materiel Laboratories (USAAVLABS) and Cornell Aeronautical Laboratory, Inc. (CAL), believe such a meeting among technical specialists in the field would be timely. It is hoped that this symposium will, through the presentation of selected technical papers, establish the state of the art of aerodynamic analysis in the basic problem areas and spotlight those critical areas where research is urgently needed. The ultimate objective is to identify those advances required in the state of the art that can assure the availability of the analytical tools needed to develop and analyze the next generation of helicopters and V/STOL aircraft.

In keeping with these objectives, five technical sessions, each dealing with specific basic areas of aerodynamic analysis associated with V/STOL aircraft, were formed.

The formal presentations of these technical sessions will be found in Volumes I, II, and III of these Proceedings. The introductory statements by the respective session chairmen and the interchange of ideas which resulted from these formal presentations are included in Volume IV.

In addition, 10 outstanding members of industry and government from three countries comprise two panels (Technical Session VI) which presented their recommendations for areas of research that need to be pursued if the state of the art is to advance at the required rate. The formal papers of the session members, along with attendant question and answer periods and summaries by the panel chairmen, have also been included in Volume IV. It is hoped the Technical Sessions will

stimulate discussions and serious thought between the attendees and the technical members of the various organizations who were unable to attend. These Proceedings have been prepared in order to foster this objective.

The cosponsors of the symposium are grateful to the many people who contributed to its success. In particular, our thanks go to Colonel Harry L. Bush, Commanding Officer of the U.S. Army Aviation Materiel Laboratories, and Mr. Ira G. Ross, President of Cornell Aeronautical Laboratory, Inc., who opened the sessions; to Mr. Charles W. Harper, our keynote speaker; to Dr. Ralph G.H. Siu, Deputy Director of Development, U.S. Army Materiel Command, for his address at the symposium banquet; to the five session chairmen --

Arthur Jackson, Hamilton Standard

Franklyn J. Davenport, Vertol Division of the Boeing Company

John W. White, U.S. Army Aviation Materiel Laboratories

Irven H. Culver, Lockheed-California Company

Seán C. Roberts, Mississippi State University

and to the two panel chairmen --

Larry M. Hewin, Technical Director, USAAVLABS

Harold A. Cheilek, Vice President - Technical Director, CAL

and, most especially, of course, to the authors and panel members without whom there could not have been this symposium on low-speed aerodynamic problems. We are indebted to the authors for preparing their manuscripts in a form that could be directly reproduced. This material was published as provided by the authors and was neither checked nor edited by CAL or USAAVLABS. The question and answer periods, along with the remarks by the session chairmen, have been carefully transcribed and verified against tape recordings of the sessions. Editorial comments are provided in brackets where useful for clarification.

SYMPOSIUM TECHNICAL CHAIRMEN

Richard P. White, Jr., CAL
John E. Yeates, USAAVLABS

CONTENTS

VOLUME IV

IRA G. ROSS, PRESIDENT, CORNELL AERONAUTICAL LABORATORY, INC.

It is indeed a very real pleasure to welcome you here on behalf of the [Cornell Aeronautical] Laboratory. Apparently, you're going to be welcomed quite frequently. Dick [Richard P. White, Jr.] has welcomed you, I welcome you, and I am sure the Colonel [Harry L. Bush] welcomes you . . . You're really welcome.

I am very happy that this group has been gathered together, because those of us who concern ourselves with military weaponry, military technology are always concerned about maintaining some sort of a balance between the various responses that are available to the military. Indeed, those of us who are concerned with the application of technology, and civilians in all fields, are concerned about some sort of a balance in the use of technology. This is very hard to maintain, because the styles come and go — the styles go for heavy missiles and the styles go for one thing and another and then they come back to pick up things that haven't been used for awhile. This is all a very healthy thing, I suppose. It keeps technology stirred up and gives new impetus and new interests from time-to-time; but, the danger is that you lose certain elements of art, certain elements of knowledge that aren't written down, that aren't recorded, that are in the minds of a group of people.

It seems to me that it is singularly important that this kind of group be gathered together from time to time. In the minds of the people in this room there is probably a great deal of information which can be exchanged man to man — which isn't recorded history of technology. It is a very important part of the very important field that you are going to discuss today.

So, again, I welcome you, and I hope that you will have a successful interchange. I am happy to hear that it started out on a delightfully informal note last evening. I hope it stays that way. Welcome Gentlemen.

COLONEL HARRY L. BUSH, COMMANDING OFFICER,
U.S. ARMY AVIATION MATERIEL LABORATORIES

Thank you, Dick . . . Mr. [Ira G.] Ross . . . Gentlemen. I would like to second the welcoming remarks of Mr. Ross. I don't feel I am fully a part of AVLABS at this moment (however, I think that it will grow on me) being there only forty-five hours . . . I reported in for duty on Monday. It is one opportunity I have always looked forward to, and I am certainly happy to be with AVLABS and to be here to talk to you for a few seconds this morning.

If you will bear with me, I will stick to a few notes that I have here to make sure that I am on the right grounds. In recent years, the Army has assumed — and I do not feel that we are boasting — the leadership and responsibility of many facets of aeronautical research and development which give promise of improved low-speed flight capability. As a part of this effort, the Army has implemented a broad program of research to study the low-speed aerodynamic characteristics of wings, bodies, rotors, propellers, and propulsion systems, both singly and in combination. This work is carried on through Army sponsorship of research programs with industry, non-profit organizations, colleges, and universities.

The Army's interest in the Symposium we are having here stems from a desire to provide those concerned with low-speed aerodynamics an opportunity to acquaint themselves with recent research findings. In addition, it will provide the specialists in the field an opportunity to exchange information, ideas, and experiences.

The interest in low-speed aerodynamics — as indicated by your presence here this morning — is most gratifying. We are particularly grateful to those who have prepared papers to give to us during the next two days, knowing that each was prepared at considerable personal sacrifice.

I sincerely hope that during this three-day period you will give us your objective comments on the information presented, because we feel that only by this medium can the technology derive the maximum benefit.

Again I would like to say on behalf of the Aviation Materiel Laboratories, and the Army, we welcome you here most heartily. Thank you.

CHARLES W. HARPER, DIRECTOR
AERONAUTICS DIVISION, OFFICE OF ADVANCED RESEARCH AND TECHNOLOGY,
NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

Charles W. Harper, was appointed Director, Aeronautics Division, Office of Advanced Research and Technology on October 11, 1964. In this capacity he is responsible for NASA's research and advanced technology program needs in the field of aeronautics which includes such areas of research as aerodynamics, loads and structures, operating problems, and propulsion. Prior to this he was Chief of Full Scale and Systems Research Division, Ames Research Center.

Mr. Harper was born in Canada on September 24, 1913 and became a United States citizen in 1941. He graduated from the University of California at Berkeley, California in 1941 with a degree in Mechanical Engineering, Aeronautics Option. He joined NASA Ames Research Center in June 1941. Successively he became Assistant Chief of 7 x 10' Wind Tunnel Branch, Assistant Chief, then Chief of 40 x 80 Wind Tunnel Branch, then replacing H. J. Goett as Chief of Full Scale and Systems Research Division until transferring to Headquarters October 11, 1964. This division has specialized in wind tunnel and flight research directed at achieving low flight speeds, in the development and use of ground based simulation as an aerospace research technique, in guidance, navigation and control system studies for manned and unmanned aerospace vehicles, and in the physical research associated with guidance, navigation and control systems.

Mr. Harper is a member of the Tau Beta Pi, Sigma Xi and AIAA.

Mr. Harper is married, no children, and lives at 2522 Queen Ann's Lane, Washington, D.C.

KEYNOTE SPEECH
by
CHARLES W. HARPER

I think it is safe to say that V/STOL aircraft, including the helicopter, have now finally come of age. Discussions over the past year, formal and informal, related to design, operation, and economic and social benefits of V/STOL aircraft have indicated that not only is the potential of this type of transport being recognized fully, but also the technological development difficulties are being now treated with due respect.

The analogy has been drawn that, in the last 6 or 8 years, the V/STOL aircraft has been to the transport industry just as girls are to a young boy. In both cases very attractive features can be recognized in a new object of interest, but the way in which advantage could be taken of them has not been at all clear. Just as the boy learns eventually that brute force will not solve the problem but that success is achieved through a sophisticated and often expensive approach, so the V/STOL user has finally realized that a simple, cheap approach is not going to lead to success. In both cases, however, substantial satisfaction should follow successful solution of the problem.

It is probable that a great deterent to the successful development of operational V/STOL aircraft was the assumption that they would be a direct extension of conventional aircraft; that is, simply install enough thrust to rise straight up, and instant V/STOL would result. Nothing seems to have been further from the truth.

The inherent magnitude of the problem is apparent if one considers only the speed ratios involved; that is, the ratio of maximum to minimum speed of the aircraft. In 30 years, we have moved from about 2 to something a little over 4, perhaps, for transports. With the SST, we plan to reach a speed ratio of perhaps 13 or 14. The V/STOL aircraft, of course, has an infinite speed ratio. The fact that supersonic speeds may not be involved changes the details of the problem but does not necessarily make it any less difficult.

Efficient performance is, of course, a mandatory ingredient for any successful transport. Unless this is achieved, the solution of any other problem is obviously academic. In most forms of transportation, efficiency becomes critical only in one phase of its operation, and this is taken as a design point at the sacrifice of efficiency during off-design operation. The SST has highlighted the unacceptability of this approach for advanced aircraft. Since about one-third of the fuel is used during climb and acceleration to cruise, this off-design operation must be carried out with high efficiency. This in turn forces a very substantial increase in the complexity and sophistication of the SST and its power plant. The V/STOL aircraft carries this problem to the extreme. High efficiency is required at both ends of the speed range if the design is to be really useful. Efficient power-to-thrust conversion must be accomplished for the thrust-weight ratios greater than 1.0 in hover and for thrust-weight ratios perhaps as low as 0.1 in cruise. This is no simple problem. Evidence of its importance and concern over it can be seen, for example, on the number of papers in this Symposium related to the propeller. I would venture a guess that no one would have predicted 10 years ago that this level of attention would be turned again to the propeller which already had some 40 years of research behind it. The same comment could be applied to other subjects of discussion in this and other recent V/STOL symposia. We in NASA consider that this return to the study of old problems is both encouraging and necessary. It is abundantly clear that many of the engineering compromises we accepted, in the case of conventional aircraft, simply won't do for V/STOL aircraft. Because these problems have been the subject of intensive and often unsuccessful study in the past, it is also clear that the solutions required will not come particularly easily.

Another return to the past which seems to be staring us in the face in connection with V/STOL aircraft is the problem of mixed or transonic flows. In the late 1940s, when the sonic barrier still existed, there was a considerable amount of effort by the country's outstanding talent directed at the study of mixed flows. Following collapse of the sonic barrier, most of this effort diverted to other work. Some of it can now be found exploring the flow field

around the earth as it sweeps through the plasma from the sun; our scientific understanding of this mixed flow problem is proceeding rather nicely. However, probably nothing is as key to the success of V/STOL aircraft as the generation of very high lift at very low flight speeds. This is true whether one is thinking of engine compressors, inlets, propellers, rotors, wings, or flaps. Generation of this lift invariably leads to creation of regions of supersonic flow embedded in subsonic flow. We know this happens, and we know the lift is increased, but the phenomena is not understood well enough to include it yet as part of a real design procedure. To obtain this understanding will require a reactivation of transonic flow studies long since dropped.

Consideration of these V/STOL problems is not offered with any air of discouragement. Certainly they can be solved when they are recognized, and the kind of talent represented here today is that required to find the solution. What is most important is that it be recognized that V/STOL aircraft will in no way represent a return to the simpler days of aviation but instead a step forward into very advanced technology.

The extent to which this advanced technology finally will become involved is emphasized even further when consideration is given to the operational use of V/STOL aircraft. It can be stated, almost a priori, that the more clearly a problem can be defined and the more sharply the constraints on acceptable solutions can be drawn, the simpler it is to find the best solution. The Apollo mission can be considered an example of a very precisely defined problem with very clear restraints. Even though the technology required to solve the various parts of the problem may be very advanced, there is very little uncertainty about the nature of the problems. The SST represents a more difficult problem in this regard. In a gross sense, it is being designed as a replacement for the long range subsonic jets to provide greater productivity and hence more favorable economics. To this extent, the technical problem to be solved is clearly defined. But the overall problem contains some uncertainties that force consideration of alternate modes of operation. Finding a solution of the technical problems so these alternates can be accomplished satisfactorily is not so easy. For example, the public reaction, if any, to the sonic boom will affect significantly the operation of the SST.

Any major change in the subsonic jet traffic pattern and routes will affect significantly the operation. For these, and many other reasons, the SST must contain within its design sufficient flexibility to operate successfully in several different modes. In this sense it is a more difficult technical problem than Apollo — although, perhaps, the Office of Manned Space Flight may not agree — although, to be sure, the solution, we hope, is not going to be as costly.

V/STOL aircraft carry this problem one step further. With almost no past experience to guide us, we can only hypothesize as to the operational patterns that will finally evolve for either civil or military use. This circumstance undoubtedly lies behind the inability to choose between the many competing concepts, each demonstrably best for one particularly attractive mission. One of the greatest challenges to V/STOL technology, then, is to realize maximum flexibility of operation in any given design. The degree of flexibility required seems to be substantially greater than achieved on any aircraft yet built.

Consideration of just one aspect of this serves to illustrate the problem. Surely the V/STOL aircraft will operate from conventional as well as VTOL ports. Since payload is the key to productivity, the V/STOL aircraft may want to take off conventionally from a conventional airport for maximum payload. But the destination may preclude this, depending on whether it is a VTOL-port or STOL-port or a conventional airport and whether it is far enough to reduce measurably the fuel load or so close as to make this effect negligible. It is not difficult to conclude, then, that the V/STOL aircraft must be able to trade take-off and landing distance for payload in a continuous manner if maximum flexibility is to be achieved. This requirement alone will aid in identifying the most promising concepts and the technological problems which must be solved to make these concepts practical.

But this requirement also poses some interesting questions whose answers are not at all evident. What happens to our classic and invariant take-off and landing parameters such as minimum control speed, rotation speed, or minimum approach speed. These now become a function of payload and not

only must the pilot be able to operate the aircraft safely over a wide range of them, but also he must know which to use for each take-off and landing and be prepared for a wide variance between each take-off and landing. How and by whom are these constantly varying flight plans to be established? Will it be done by some form of ground control or must the pilot be given an on-board real-time picture of his constantly changing flight envelope so that he can make decisions to meet any set of circumstances. It is rather easy to predict a system sophistication at a level above anything we now see, even including Apollo. The recent study made by Rene Miller for the Department of Commerce certainly highlights this problem.

Great as the challenge may be, there is little reason to doubt the value of meeting it. Any questions in this regard can be answered by examining the effect of introducing into Washington National Airport the short-haul jets. Here was a successful operation based on serving an area within about 650 miles of Washington and using aircraft which were becoming, in a sense, unsuited to other parts of the systems. The jets were introduced, at least in part, to enable uniformity of equipment throughout the various systems. Apparently no one really envisioned the resulting impact on air travel. In less than two months, it has become necessary to limit the service not to what the public traveler wants but to what the ground services and the air space can allow before becoming saturated. If ever an example existed to support the contentions of the V/STOL enthusiast, this seems to be it. But it also adds a warning. Unless the V/STOL aircraft and its ground control system contain very great flexibility of operation, air space will saturate and, like our freeways, the potential time savings will be lost through traffic delay.

Symposia, such as this one starting today, are a very encouraging sign that the V/STOL potential and its technical problems are being given the attention they deserve by the kind of technical talent required. The NASA is looking forward to working with the universities, with the services, and with the industry in bringing this new capability to an operational stage.

Thank you.

DR. RALPH G.H. SIU
DEPUTY DIRECTOR OF DEVELOPMENT, ARMY MATERIEL COMMAND

Dr. Ralph G.H. Siu was born in Honolulu. He received a Bachelor of Science degree from the University of Hawaii, and a Doctor of Philosophy degree in Chemistry from California Institute of Technology. He formerly served as a research fellow at Cal-Tech, as a chemist in the Department of Agriculture, as a research associate at Harvard University, as a technical director of the U.S. Army Quartermaster Corps, and, more recently, as the scientific director of the Research Division, Army Materiel Command.

He is the author of many books, papers, and patents — principally in the area of chemistry and management. His latest book is a philosophical essay, published by the MIT press, entitled "The Tao of Science."

He is the recipient of the Army's Exceptional Civilian Service award and the National Civil Service League award. Dr. Ralph Siu is now the Deputy Director of Development, Army Materiel Command.

Banquet Speech

THE RITUALIZATION OF THE SCIENTIFIC METHOD

by

R. G. H. SIU

I. INTRODUCTION

I bring you General Bunker's regrets that, because he is not feeling well, he has to forgo the pleasure of being with you tonight. He wishes you a very successful conference on a subject of great interest to the Army. As I glance over the agenda, I recognize names associated with some of the most important recent advancements in V/STOL. We in the Army only wish there were many more like you.

Conferences of this kind are very helpful in many ways. One of the most important, I believe, is providing the opportunity of surveying a given field of activity — a reassessment of our state of knowledge and validity of insights. I would like to join this spirit of sitting back and taking a detached view. In tonight's short talk, I would like to sit far back and take a long look not at aerodynamic problems associated with V/STOL aircraft, not at the enveloping science of physics, but at the broad issue of whither will science go from here.

I have a vague feeling that there is a kind of ritualization of the scientific method setting upon our approach to human problems. While I feel the method is fine for V/STOL, I am not so certain it is good for the many other activities which are being enamored with its contributions in the materiel arena. I would like to discuss this as yet ill-defined, nebulous sensation with you tonight.

II. THE SCIENTIFIC METHOD

There is a vague suspicion that a major advance in man's approach to the understanding of nature is being shaped today. It has not quite emerged as yet and its form is not yet clear. But I can see the prevailing methodologies giving way here and there.

We can start our discussion with the effectiveness of the scientific method. There is no question that science is regarded by most people today as the authoritative source for the understanding of nature. In view of her commanding position, we may ask:

> How satisfying are the explanations of science? How much further can her capabilities extend to provide the answers useful to the essential questions of life?

We can obtain a sense of the situation through an example from the field of physics.

Around the turn of the twentieth century, Rutherford and Bohr presented their model of the atom with its neat arrangement of protons and electrons. The two particles were supposed to be the homogeneous fundamental building blocks of everything — the so-called elementary particles of nature.

This achievement of the scientific intellect was widely acclaimed. By means of the scientific method, man was extending his understanding of nature into the very interior of the atom.

In the decades following this auspicious beginning, nuclear physics began to make great strides. Discoveries piled on discoveries. Newer theories displaced older theories. Unfortunately, this surge of findings has not brought us any closer, it seems, to certainty in our knowledge of the atom. As a matter of fact, we find ourselves at a rather perplexing crossroad.

Instead of the simple atomic picture of two kinds of elementary particles, as we were once told, there are now supposed to be over 40. And the list doesn't appear complete. The Brookhaven workers, for example, tell us that the neutrinos are of different kinds. Instead of the electron being a uniform building unit, as we were once told, it is now supposed to have a morphological structure. And to top it all off — when Hofstadter got his Nobel prize for his brilliant work on nuclear particles, he stated that perhaps there are really no such things as elementary particles after all!

A lay observer to this state of affairs may well ask:

> Is the scientist getting confused? Is he drifting farther and farther away from the joy and the laughter of the living real world into the monastery of scientific virtuosity? Can he solve the meaningful problems of nature alone? Or does he require a new perspective?

The scientist would respond by assuring him that these continuous adjustments constitute the very strength of the scientific approach, that it only appears that the picture is getting hazier, and that it is actually becoming progressively clearer as more of the heretofore unknown factors are uncovered.

But the situation is not as straightforward as all that. Let me illustrate the point with three little stories.

The first concerns the law of cause and effect. It lies at the base of scientific work and has served science well. Actually, though, the law has never been proven decisively. This is illustrated by the story of the little chicken which ran away in fright at its first sight of a man. After the man left, the chicken came out of its hiding place only to find some corn on the ground. This was repeated over and over again — 999 times. In terms of the law of cause and effect, this would mean that, whenever the man appeared, the corn must also appear. So when the man came out the thousandth time, the scientific chicken ran out to thank the man, only to have its neck wrung for supper that night. Obviously the assumed law of cause and effect failed the chicken miserably the last go-round.

The second story concerns logic. Scientists pride themselves on being very logical people. But, we must be careful about trying to follow logic to the bitter end.

This is illustrated by the story of the beautiful maiden who fell out of her canoe into the hands of the king of the alligators. The mother tearfully begged for her return. The alligator king was actually a good fellow; so he gave the old lady a sporting proposition.

"I will return your daughter if you can make one true statement," he said to the mother.

Without thinking, the old lady said, "You are going to keep my daughter!"

The thinking man frequently finds himself in the same fix as the alligator king. If the statement that he is going to keep the girl is true, then he should return her. If the statement that he is going to keep the girl is false, then he should keep her.

So you can see that he's damned if he does and damned if he doesn't. Scientists don't like this kind of dilemma, although we in Washington are quite used to it.

The third story concerns the process of reasoning itself. Science feeds on reasoning. But there are many occasions in which reasoning is not an effective tool. It is frequently difficult, for example, to appeal to a neutral principle in order to arbitrate between the rationality of competing systems. This is revealed in the story of the Mormon student who submitted a critical thesis on Mormon history. One of the members of the thesis committee asked the student:

> "Do you think that, being a Mormon, you are sufficiently unprejudiced to write a fair account?"

Whereupon the student replied:

> "Yes, if you, not being a Mormon, are sufficiently unprejudiced to judge it."

These stories provide some indication of the reasons why some people do not believe that science can provide reliable guidance for an understanding life. There are too many ways in which conventional logic traps a person in a paradox of some kind from which the very logic permits no escape.

III. BASIC RESEARCH

Despite such considerations which ought to emphasize the essentiality of selectivity in the application of the scientific approach, there is a ritualization of the scientific method which is extremely difficult to resist nowadays. This is not without some danger.

The philosopher Whitehead has reminded us that "Some of the major disasters of mankind have been produced by the narrowness of men with a good methodology." Men of stature have always been able to decide when it is appropriate to use the techniques in which they are expert, when they should be discarded, and when new methodologies should be developed.

I would like to discuss just one example which is close to our hearts — it is the so-called basic research in the scientific laboratory.

Although it is fashionable to talk about basic research nowadays, it is not an invention of modern times. As far as its essence is concerned, basic research is nothing more than inquiries at the prevailing boundaries of human knowledge. There has always been a prevailing boundary, and man has always been curious. In this sense, then, basic research has always been with us.

Let us look at the basic research taking place during the period just before the birth of the scientific era. During that time, basic research in philosophy had as one of its prime objectives the support of applied philosophy, orthodox theology, and the ruling social order. It sought answers, consistent with traditions, that have been built up by the philosophical techniques. It attempted to strengthen medieval philosophy as a tool for understanding nature.

As these basic researchers delved deeper into the ways of nature, however, they began to find that the methods available were no longer capable of providing satisfying answers. If their intellectual integrity was to be satisfied, new methodologies were required. In the search for these new approaches, imperceptible step by impreceptible step, they began to undermine the very methodology they were supporting as the dominant means of enlightenment. As a matter of fact, many of them, such as Descartes, recognized what was happening and were never fully at ease with their lot in breaking away from tradition.

I wonder whether or not the truly pioneering researcher in the scientific laboratory today is being confronted with a comparable situation. He is trying to grapple with problems of formidable proportions: the origin of

life, the infinity of the cosmos, the nature of time. The tools of science appear so puny in contrast. Thus we observe the modern biochemist resorting to circular explanations in trying to understand the nature of life — just like the medieval philosophers used to do in trying to explain the nature of the soul. We find the modern physicist resorting to the creation of matter out of nothing to explain the heavens — just like the medieval philosophers used to do in trying to explain the earth.

So we ask ourselves: Can the modern scientist find the answers to the questions he poses by means of the scientific method as we know it? The concern becomes more acute in cases involving human behavior and decision, such as in the fields of operations research, psychiatry, and other interfaces between the sciences and the humanities.

Science, as we know her today, cannot furnish what the philosopher Northrop calls the "immediate apprehension of the totality," so that man can act now, at every instant, with an enlightened strategic grasp of nature. It cannot provide the keen awareness of living that has so far defied explicit description. It may well be that the most significant intellectual movement of the twentieth century will begin with the foremost scientist breaking away from the methodology of science itself and formulating a new approach to knowledge.

IV. CONCLUDING REMARK

In concluding this speculative excursion into the evolution of knowledge, I would like to address a rhetorical question to ourselves:

> If there does happen in our lifetime that a modern Galileo came forth with a new salient of knowledge, as a consequence of our awareness of what happened to the earlier Galileo, will we be more receptive than were our ancestors. I wonder. Anyway, I sure hope so.

TECHNICAL SESSION I
WEDNESDAY MORNING, 22 JUNE 1966

SESSION CHAIRMAN

A. JACKSON,
Hamilton Standard Division of United Aircraft Corporation

I would like first to thank Cornell Aeronautical Laboratory in general, and Dick White in particular, for extending to me the honor of chairing the first session of this V/STOL Symposium. Furthermore, it is a particular pleasure to me, as a member of a company still very actively engaged in the engineering and marketing of propeller systems to see the technical problems associated with this device discussed as a sort of Chapter 1 in this three-day program, rather than being relegated — as perhaps some would prefer — to the back appendices. This treatment represents to me the type of attention the propeller systems must receive if they are to be allowed to develop their full potential as part of a superior propulsion system for various types of V/STOL aircraft.

Now, I am sure there are few people in this assembly who are unaware of the fact that most of the propeller-driven VTOL aircraft flown to date have experienced, at least in their early development stages, problems of varying degrees of severity which have to do with propeller performance and structural loading in the hover and low-speed flight regime. Of the two, the performance problem has been generally the most severe and, perhaps, the most fundamental.

In either case, the difficulties have been to a large degree the direct result of inadequacies of existing analytical theories for accurate predictions of propeller performance and propeller loading.

The need for better analytical techniques in the low-speed propeller aerodynamic area have been recognized for some time. Attacks on the problem have been mounted by a number of organizations in government, universities, and industry.

The most significant in this regard has been the work performed by Therm Advanced Research, Inc.

Now, to introduce the authors of the first paper, Dr. John Erickson and Dr. Donald Ordway. Mr. Erickson will make the presentation.

Jack Erickson was graduated from Cornell University with a Bachelor of Engineering Physics degree in 1956 and subsequently received a Doctor of Philosophy degree, also from Cornell, in 1962.

From 1962 to the present, he has been senior staff scientist of Therm Advanced Research, Inc. Prior to that, at Lewis Laboratory, Dr. Erickson assisted in an experimental research program on the aerodynamic noise generated by jets and boundary layers. His graduate work was spent in research on unsteady jet-flap theory. At Therm, he has been a principal investigator in propeller aerodynamics, particularly on the static thrust problem. He has also continued work in the jet-flap area and has carried out studies on lifting reentry bodies and nonlinear stability of V/STOL aircraft.

Don Ordway graduated from Cornell in 1953 with a Bachelor of Engineering Physics degree with Distinction. He received his Doctor of Philosophy degree from Cornell in 1956.

From 1957 through 1959 he was an Assistant Professor at the Graduate School of Aeronautical Engineering, Cornell. Subsequently, he helped found Therm, and, currently, from 1964 to present, he has been Vice President of that organization.

As I mentioned earlier, Jack will present the paper, and I will turn the meeting over to him at this time.

A THEORY FOR STATIC PROPELLER PERFORMANCE

by

J.C. ERICKSON, JR. and D.E. ORDWAY,

Therm Advanced Research, Inc.

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN JACKSON: For your questions, would you first identify yourself — your name and your organization?

QUESTION: Raymond [W.] Prouty, Lockheed.

I am not sure if these are questions or not, but I do have a couple of comments.

It seems to me in trying to match the pitch of the vortex sheet, you are missing a good bet in that there have been several methods of flow visualization developed to see where the trailing vortex goes through these rotors. IBM suggests — although I've never seen this work — that you put a Fourth of July sparkler on the tip; time exposures will then represent velocity vectors of the sparks. But, anyhow, there are some good flow visualizations which I think will pin down where that tip vortex is and what the pitch of it is.

MR. ERICKSON: Yes, it will help, and I think it's been a case that sometimes we haven't had the visualization tests of the same propellers we are making the calculations on; perhaps there are more generalities than we might expect at first. But, this is a good point, and I am going to hear some more this morning later, I know, about some of these visualization tests made -- especially at Canadair. This is a good source of an additional check. In fact, it was with these visualization results that we concluded that our general contraction pattern was adequate.

MR. PROUTY: The other point that I think is worth mentioning is that, for a prop with a finite chord at the tip, there is bound to be three-dimensional effects, and the way that circulation falls off to the tip is probably an important thing in defining. I imagine a marine propeller would be even worse.

MR. ERICKSON: Yes, this is certainly true. In fact, you've hit the heart of a very sensitive parameter there. The exact falling off of the circulation distribution at the tip is extremely sensitive in the calculations. You make a very small change there, and you can make quite a big difference in the predicted in-flow values; mainly, just locally, but, nevertheless, it is that tip region which is very important. I would mention that these lifting surface effects are certainly important, and, ultimately, they probably should be considered.

One encouraging thing I think along these lines is, you look at the marine propeller literature in recent years — this work of Kerwin's [Reference 13 in presented paper] which I mentioned briefly and other work — and you find that the basis of their theories is that they start with a lifting line solution and make lifting surface corrections to it.

So, I see no reason why we couldn't use this lifting line theory — which differs from their lifting line theory only in that we are accounting for the deformation — and using this as a first step and then make lifting surface corrections to this. This is certainly something that probably, ultimately, should be looked into, I agree.

QUESTION: Frank [Franklyn J.] Davenport, Vertol Division of Boeing.

First, I would say I enjoyed the presentation. It is one of the most fiendishly difficult problems of fluid mechanics that I know of. I was very interested in your vortex structure as you showed it there in your refined approximation . . . how it had a much tighter pitch. I wondered if this was also true in the interior of the wake?

I've noted that tip vortices tend to travel at roughly half the inner velocity inside the slipstream. I wondered if this reflected that [condition].

That is one question . . . and I have a couple more and then [I will]sign off.

I presume you use control stations along the radius in order to compute these things in some finite form. I would like to know, if you don't mind, how many you use and how you distribute them? I guess those are my only questions.

MR. ERICKSON: Okay. On that first one, you are asking what the pitch variation — what the typical pitch inboard [is] as opposed to that at the tip which we were looking at. Could I have the last slide, please [Figure 5, page 16 in prepared paper.]

The pitch which we saw was dependent on the very low velocity here [author pointing to slide]at the tip. The typical actual velocity distributions are falling off very rapidly like this [pointing]and we assume that the pitch is determined by this value here [pointing]. Well, actually the value as shown on an earlier slide was actually from the previous approximation [in the iteration scheme] which is a slightly larger value than this one, which is very close to zero. The pitch at each point inboard of that starts off just based on the actual velocity at that point. In other words, it is increasing quite rapidly from the tip.

So, say, by the time we are at 0.9 stage[span] in here [pointing], we have an actual in-flow of 0.05 [Figure 4] as opposed to something which is like 0.005 perhaps down here [pointing]. The pitch here [pointing] is that much greater and then increases and is generally of this level [pointing] over the interior of the blade, and the pitch there at all of these radial stations [pointing] begins with a value based on this in-flow here [pointing], and we assume that it doubles as it proceeds back downstream and has doubled, in fact, by the time it reaches 1-1/2 radii downstream.

So, in this sense, the pitch is increasing quite rapidly as we proceed inboard just because the actual velocity typically comes out looking like this [pointing] at least for the low pitch setting cases — [for] the higher pitch settings, it is not so pronounced. That is why the differences will not be so great between the two rate hypotheses.

Now, to answer your second question. We are performing the integration numerically and we have

to calculate the influence functions — say, the u_t — for a number of vortex elements representing the sheet; I think your question was: How many vortex elements do you have to use?

Basically, what we have been using are 24 basic elements. Then, if you have a Glauert variable that runs from zero to 180 degrees, it's like every 7-1/2 degrees. Furthermore, to get an accurate calculation of the regular parts of these influence functions around the singularities — i.e., when the vortex element is near a field point at which you're calculating the in-flow — for that situation, we bunch the points; actually, in most of these, we have two points right on each side to give better definition of the regular function there. Now, in further work, we are limiting that just to one because it seems to be relatively smooth.

So, for most of the calculations that we had made, we found that we were using on the order of 40 to 50 trailing vortex elements. Now we feel we can reduce that number, because there is going to be a good savings in time; and, in fact, we are going to have the input a little more general to be able to put [it] in a little more easily. We had to go through quite a bit of work to put the locations of all of these elements in — read these data in — in the first go-around of this thing. We are going to have it much simpler so that we can, in fact, vary the number of terms and compare just what sort of changes are involved.

I know [in] some of Kerwin's work, he got pretty good results from marine propellers using like eight or sixteen trailing vortex elements — on this order of magnitude. So, I would hope that in ours, which is different in detail, we could perhaps reduce it to a similar amount.

QUESTION: Frank [D.] Harris, Vertol Division of Boeing.

If I remember back, the ideal propeller is one that has a constant downwash or which is a constant axial velocity? Is there any reason to expect that all of this work in the vortex placements and pitch and what-have-you is going to significantly alter the pitch distribution that I want so that I could get uniform downwash? Presumably, the best figure of merit would come with this, and how much am I going to change my opinion about a pitch distribution to achieve it?

MR. ERICKSON:

That's a good question. This is a question of optimization — that is really what you are asking: How do we optimize the design? As you indicated, in forward flight, if you can get this constant displacement velocity, [which is] the usual terminology, then you have at least the optimum induced pe.formance of a propeller. That is, of course, based on little or very slight amount of contraction and actual deformation as you proceed back.

I don't know what the optimum condition is going to be in the static case. I think you are just going to have to go back to fundamentals and, to really find the optimum, [you will have] to write down your integral expressions for thrust coefficient and power coefficient and use calculus of variation techniques to optimize the thrust for a given power coefficient.

But, that is going to be a really difficult problem in itself. I think it is only going to be through the use of somewhat simplified theories to evaluate the resulting equations which would come there that you are going to at least get any kind of a real crack at what would be the optimum performance. And, this would just be the optimum [induced performance], although you could do either optimum induced performance or even optimum real performance if you could include your profile drag effect. But, at the present stage, I don't really know the answer to the question.

QUESTION:

Al [Aldo] Peracchio, Hamilton Standards Division.

I was wondering if you could comment on the difference in answers you got with the two damping factors. Shouldn't they both converge to the same [result].

MR. ERICKSON:

[Interrupting:] Oh, that was not a converge solution. That was just a single approximation.

MR. PERACCHIO:

Oh, O.K.

MR. ERICKSON: I mean, we hadn't carried it through. We just put those points on there . . . it was the first try to see what direction do we go — are we improving things at all. It is not to be taken as an answer. When you get a solution, it doesn't matter what you use for that factor. The term within the square brackets which that factor multiplies would be identically zero as your solution.

QUESTION: George Kurylowich, Boeing.

How did you get around the fact of mutual interference in the wake — this business of core size and so on?

MR. ERICKSON: George, we assumed that we are dealing with vortex sheets — infinitesmally thin vortex sheets . . . in other words, continuous sheet distribution of vorticity. We did not have discreet vorticies of any kind in the wake so you really are not concerned with a core size.

MR. KURYLOWICH: Didn't you originally have a pitch-fork model?

MR. ERICKSON: Yes, in our original work, which we did for Curtiss-Wright — which is not really too closely related to what we had here — we had a continuous distribution for a distance and then an immediate discontinuous contraction to a single discreet vortex which was far enough downstream so that, again, we did not have to worry about the core size — we just considered it as a line vortex, because its influence back up at the lifting line was relatively small — the reason that the effective core size would be relatively small.

MR. KURYLOWICH: Why did you throw that solution out?

MR. ERICKSON: Well, for a number of reasons. First, that [solution] assumed actually the initial continuous deformation had been regular helical vortex elements; in other words, it was not a smooth contraction. This appeared to be a really serious drawback to the problem. You had to represent

this initial deformation immediately behind the propeller, the smooth contraction of your trailing vortex sheet. Now, we could have put that in, admittedly, with the discrete vorticies downstream, but we didn't feel that we really knew enough about the vortex dynamics back there to really predict where this vortex could be and what its strength is.

I mean, now, with this trailing vortex sheet which we are considering here, of course, there is a concentration of vorticity near the edge of the sheet; and, as it contracts and deforms, it gets squeezed down so that there is even a further concentration of vorticity out there near the tip. So, in a certain sense, we are having a representation of a concentrated amount of vorticity, but we're still taking it in sheet form. We just decided to proceed along these smooth continuous deformation lines for the present and just see how far we can go with that; if actual discrete vorticies had to be introduced later, they could be. We could include it in our model if we wish, but we haven't seen fit so far to do that. Does that answer your question, George?

MR. KURYLOWICH: Pretty well. I seem to be a bit confused. How do you represent roll-up?

MR. ERICKSON: We haven't explicitly represented roll-up here. We've got deformation of the sheet and, with that deformation, a certain amount of concentration of vorticity but not roll-up discretely into something which you could just say is discretely in vortex as opposed to being part of a vortex sheet.

MR. KURYLOWICH: Thank you.

QUESTION: Norman Gorenberg, Lockheed.

In consideration of a comment that was made earlier or a question presented to you about the optimization of a propeller for performance, and particularly with regard to the hovering case or the static case, I think there is enough evidence right now to indicate that we are going to be somewhat short of a uniform induced velocity without having to be too concerned about the particulars which establish that.

First of all, for the point of view of the static case, if minimum power is to be achieved with the constant or uniform induced velocity, it is fairly evident from test work (and even some of the more simple approaches to the propeller) that the inboard sections will not support a uniform induced velocity; and, by simple examination, it is pretty evident that it is going to cost at least seven percent more power (. . . a seven percent increase in power) in the induced velocity region at the very best.

I think this is an adequate gross answer for that aspect of the problem.

QUESTION: Andy [Andrew R.] Trenka, Cornell Aeronautical Laboratory, Inc.

I am interested in finding out how you determined when you had reached a converged set of velocity values in your iteration procedures.

MR. ERICKSON: As I said, in the refined wake hypothesis, we hadn't actually gotten there. We are dealing with an integral equation, and I think the best you can really expect to do is to achieve convergence in some sort of a global sense — an integrated sense, if you will — [where you] integrate the error over the distribution of the in-flow and try to take that down to within acceptable levels.

MR. TRENKA: What sort of level do you call an acceptable level? Did you find that, say, a few percent error in your velocity distribution [resulted in] a significant change in the [vortex] strength close to the prop plane?

MR. ERICKSON: As long as you are getting things represented pretty well out toward the tip — in other words, the axial velocity had settled down pretty much there — I think performance would settle down by that time.

QUESTION: Hank [W.H.] Tanner, Bell Helicopter.

This is sort of a comment on a comment. In the case of the static thrust, the induced velocity, and the circulation distribution, we have been doing

quite a bit of experimental work as well as analytical along this field — and we'll have something to say about this Friday morning. I believe our work is just as applicable to the static thrust case for the propeller as for the helicopter rotor, which, of course, is our main concern. Thank you.

QUESTION: Dick [Richard M.] Ladden, Hamilton Standard.

Just one question; then I would like to make a comment. Do you still plan on putting the airfoil data into the computer program, and could you give an estimate of about what you feel at this point that streamlining of the program will do in terms of computer time?

MR. ERICKSON: We have put the airfoil data in in a rather brute fashion. Just take the airfoil data, C_L versus α, C_D versus C_L at each of our ten radial stations — then we read that data in, read the inflow in as computed, and then compute the performance. No, we have not got the program to a point, Dick, where we can make any estimates of the time that is going to be involved. I just mention that — I just passed over it rather briefly — we seemed to have found certain generalization of this regular part of the induced velocities which seems to give us an indication that we can proceed much more rapidly toward a converge solution.

In other words, [we do] not go through the approximations as we have been in all of the work that we have done to date and the work with which you are familiar, but rather [we] get a first approximation and then iterate, just using the singular part — which is much quicker to calculate, a matter of a couple of minutes to do a case — to get us very much closer to the converge solution, at which time we can use the full program again to get a converge solution.

This is not a computer programing simplification but rather an observation based on our results which enables us to get a simpler technique for getting close to a solution.

MR. LADDEN: The comment I would like to make concerns — reverting back to this concept of three-dimensional effects and lifting surface (this may or may not be important, it remains to be seen) — another consideration which could be important — in fact, could be even more important — are the three-dimensional effects on the two-dimensional data itself; in other words, the effects of cross flow. Certainly, [this would apply] out at the tip where cross flow is at a very high angle. That was the only comment that I wanted to make.

CHAIRMAN JACKSON

We will now move on to the next speaker. Obvious to the value of any analytical prediction method, it can only be established by accumulation of considerable substantiating experimental data. When existing theories proved inadequate in this respect, the propeller industry fell back to the next best thing . . . deriving prediction techniques by empirical means through exhaustive testing. The Curtiss Propeller Division of the Curtiss-Wright Corporation has been particularly active in this area.

The next paper describes this work and will be presented by Henry V. Borst, now of the Vertol Division of Boeing Aircraft.

Just a brief synopsis on Mr. Borst's career. He joined the Curtiss Division's Aerodynamics Section immediately after receiving a Bachelor of Science degree in Aeronautical Engineering from Rensselaer Polytechnic Institute in 1943. He advanced to Chief Aerodynamics Engineer at Curtiss, a position which he held for a number of years. Mr. Borst is an inventor of the tilt-propeller concept used in the Curtiss-Wright VTOL aircraft. His work in this field led to the design, manufacture, and successful testing of the Curtiss-Wright X-100 VTOL aircraft and the design and manufacture of the X-19.

The paper is co-authored by Dick Ladden, who also recently was of Curtiss-Wright, and more recently has joined Hamilton Standard.

He received a Bachelor of Aeronautical Engineering from New York University in 1957 and an M.S. in Mechanical Engineering from Stevens Institute of Technology in 1964.

From 1962 to 1966, Dick was a Project Engineer at Curtiss-Wright, and he handled, in the area of propulsion, responsibility for work dealing with propeller aerodynamics, engine installation, performance and internal ducting design for Curtiss VTOL aircraft designs. He was also responsible for aerodynamic propeller blade designs.

This paper will be presented by Henry Borst.

PROPELLER TESTING AT ZERO VELOCITY

by

HENRY V. BORST and RICHARD M. LADDEN
Wright Aeronautical Division of Curtiss-Wright Corporation

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN JACKSON: I am sure that we can have a lot of questions on this paper. We have quite a bit of time before our break at 10:45.

QUESTION: Harold [A.]Schuetz, Wright-Patterson Air Force Base.

I have several comments. One, to distinguish between the two rigs that you showed on the slides, there is quite a bit of difference, really. I agree with you on the blockage data that you got on rig 3. However, on rig 4, with the extension, we feel that we are getting good correlation at the present time.

One question on your rig. What potential accuracy do you look for in your thrust in your torque-measuring equipment?

MR. BORST: Let me just state that the NACA data with the extended shaft did show that the performance measured was about the same as with the other rig. The extended shaft didn't seem to do much for you. Now, this may be wrong and should be further investigated; but, that's what the data did show us.

On the accuracy, we tried to pin the thrust measurements down to one percent. We had a calibrating device that we would put on the rig of the same order of magnitude as the thrust that we were measuring. Right before and after the test we would calibrate to that dead weight. It

is awfully hard to pin these things down, but we felt this was the best way that we could think of to get accurate data. I felt it was in the order of one percent (or maybe a little bit worse than that), but that is what we tried to do.

MR. SCHUETZ: What about the torque?

MR. BORST: The torque we found was within about that same accuracy. We had the torque meter hooked up with the engine, and these things are guaranteed, I believe, within a percent and we got good correlation between the torque meter and the Baldwin [-Lima-Hamilton A-160] Torque Cell. In fact, I felt the torque was a better measurement than our thrust. This was a commercial device, and our calibrations generally were quite good.

QUESTION: [W. Z.] Stepniewski, Boeing-Vertol.

I am wondering whether you made any measurements of your downward distribution farther down in the wake between that whole variation of downwash until it comes to . . .

MR. BORST: [Interrupting:] We would have liked to but we didn't have the time nor the money. This is one of the programs that we wanted to do — to take that hot film equipment survey not only just behind the propeller, [but also] we wanted to go a half-diameter, a diameter, two diameters, and we also wanted to try to go in front of the propeller to get the performance — but we never did get that.

MR. STEPNIEWSKI: I have one more question. In the blockage effect, there is always an impression that you are getting much better figure of merit when you put some big obstacle behind. Of course, probably as far as the propeller is concerned, it feels that influence almost as, to some extent, the proximity of the ground — or something like that. But, obviously, you develop also some drag on the obstacle itself. Did you consider that interplay?

MR. BORST: Well, I can give you the results of the last bit of work that we did while I was there. We took the wing of the X-19 and mounted it behind the propeller in the same location as on the airplane. We measured the forces on the wing separately and, of course, the forces on the propellers separately. The figure of merit that we got on the propeller went up, and the results were a little bit confusing as a function of disk loading, but I can safely say, I think, that they went up three to four percent as a minimum.

The wing experienced a drag, of course, and, projecting the wing area underneath the disk as the measured value of the area, we determined some drag coefficients of that wing and found that, without the flap deflected, the wing had a drag coefficient of about 1.1 which corresponded to a download of about 15 percent. By deflecting the flap, we reduced that drag coefficient to the order of 0.7. There was a big flap; I noticed you raised your eyebrows — it was a very big flap. Then, we went one step further, and we put a lead edge device on the wing with the idea of trying to sweep some flow underneath the wing, and we brought the drag coefficient further down to about between 0.3 and 0.4.

I feel that we were starting to hit pay dirt on this and that future research in this area is desirable and will net some important results for fixed-wing type VTOL.

QUESTION: My name is [William F.] Putman, Princeton University.

Mr. Borst, I wonder if you ever tried, on the blockage experiments — particularly, say, on that 15-foot extension — to thrust in the opposite direction so that the in-flow is the cluttered flow and the wake is maybe uncluttered?

MR. BORST: That was done in the NACA results, and I'll be darned if I can remember what the results of that were. I think it approached the free propeller. [Directing comment to Mr. Richard M. Ladden:] Dick, do you remember that?

MR. LADDEN: No, I don't. It seemed to me that the effect of blowing air backwards in that data showed that there was an increase in thrust, which seems a little backwards; but as I remember, I think that was the way it was.

MR. BORST: Well, I don't remember.

MR. LADDEN: This is kind of hazy in my mind, too.

MR. BORST: The reference is in the report. I think if you can dig it out, you can see. But, that was done.

MR. PUTMAN: With the 15-foot extension?

MR. BORST: Yes! Well, with the model prop at NACA. It was done on a 10-foot prop. That was simulated.

MR. PUTMAN: Very good. Thank you.

QUESTION: Jack [John P.] Rabbott, Sikorsky.

I agree with your comments on the effects of Reynolds number on performance, but I notice in Figure 15 you show a fairly sizeable effect of a small change in twist on the figure of merit. I wonder if your model propellers were dynamically scaled — particularly in torsion — so that you would have the same dynamic twist effects?

MR. BORST: Figure 15 is a full-scale propeller test.

MR. RABBOTT: Right. But, it compares the effects of small changes in twist when you are comparing your model scale versus . . .

MR. BORST: [Interrupting:] Well, you may have a point there. However, we did some calculations, and it was felt that the twist between the full scale and the model data of the 2.6-foot model prop against

the 15-foot model prop was about the same. The model prop was a solid fiberglass prop, and it was quite rugged; the full scale was dural [duraluminum]. We conducted a calculation run and, as I recall, the change in [live] twist was about the same between the two props.

QUESTION: Glen [N.] Adams, Canadair Limited.

I think when you get quite large variations in performance of full scale propellers tested on two different rigs, as you sometimes do, you have to be a bit careful in saying that, because you get different performance in model scale, that there is necessarily a big Reynolds number; it could well be an effect of the rig on which the propellers are being tested.

MR. BORST: I agree.

MR. ADAMS: In one of your slides, you compared the X-19 propeller tested on, I guess it was the Wright-Field rig and on our rig, and showed some difference. I wouldn't vouch for the accuracy of the data obtained on our rig at that time; and, particularly, if you get the different shape of the figure of merit curve on two rigs, then I would be inclined to suspect the data.

MR. BORST: Well, yes, that was shown on Figure 13. The peak was chopped off on the model — the 5-foot model.

QUESTION: Carl Rohrbach, Hamilton Standard.

Regarding this rig blockage, we recently had the opportunity at Wright-Field to test the same propeller on essentially all of the test rigs that are located there. In each case, the plane of the propeller is at different locations relative to the front face of the rig. We tested on rig 1 with and without a gear box extension and on rig 3 and on rig 2, all with slightly different and sometimes significant differences between the plane of the propeller and the front face of the rig. We utilized the type of ground plane analysis in

Gessow and Meyers and we were able, with those data, to correlate calculations with the data and, in fact, predict the level of performance that we got on the propeller from rig 4 with the extension.

And then, going further than that, taking a look at what the propeller would look like without a ground plane showing that rig 4 at Wright-Field with the extension of something like 14 feet showed performance within a percent of what we feel the isolated propeller would be. I think this was generally confirmed when this same propeller was tested, both on a more isolated rig at Wright-Field, and that [which] would be implied from some operation we did on the airplane. That is the comment.

I have one question. With regard to the axial flows in the wake that you found — where you implied that the fact that the velocities were approaching something less than zero at the tip — I wonder if that isn't what you would expect from contraction? We found from wake surveys in smoke pictures that this was the case. There was a definite indication that, even 10 inches back, you are already severely contracting.

MR. BORST: I agree with you that the contraction was very rapid. The condensation pictures that we got showed this.

However, the only point I was t.:ying to make is that the X-19 propeller, which had poorer performance than the more conventional prop, had a smaller velocity at the tip than did the other prop, and they were loaded about the same.

QUESTION: Jack [Anton J.] Landgrebe, United Aircraft Research Laboratories.

From the condensation trails that you obtained, do you feel that they may be adequate to fuse a picture of the actual wake for theoretical techniques as far as the contraction and, also, did you see any indication of the inboard wake as well as the tip wake?

MR. BORST: No, I was there many times during these conditions and I didn't see anything that approached the inboard wake. Incidentally, the photographs . . . one technique — if you see these condensation trails on a propeller — [is to] take a good fast shot of them [and] with the pictures, you will separate them out. You can't see them with the naked eye. You see the average.

As far as your other question is concerned, the condensation trails help define what the wake is, but I don't think it is enough. I feel that further hot wire or hot film data is required to really trace it back and find out where it is going.

With the condensation trails, it is a little bit hard to define where the center of the trail is — they do jump around a little bit. Another thing, you have to be awfully lucky to get them. I spent almost a year before I found the first one. This is kind of annoying if you've got any schedule to make.

QUESTION: [F. B.] Gustafson, Langley Research Center.

Just one point that I would like to emphasize in regard to these comparisons. You mentioned that you would have put the propeller higher had you had enough money. I think perhaps there is an intermingling of effects of height above the ground with the other comparisons that you are so anxious to make.

It's been my experience that it is very easy to induce a cross flow in a propeller or thrusting rotor. I believe the effects of this cross flow may be very different according to the pitch distribution, proximity to stall, and so on. So, I think that we still have that item left over.

For example, when you get to the effects of diameter, you are farther above the ground with a smaller diameter. So, I think there is something left to unsnarl there. Do you agree?

MR. BORST: I agree. The only comment I can make here is that, looking at these condensation trails, they seem to be pretty symmetrical. I would have liked the rig to be another 10 or 15 feet above the ground. Originally, we had the idea we were

going to mount it above the ground with the prop horizontal and blow upwards; but, we ran out of money for that job and had to settle for this.

I feel that it would be a worthwhile item to take and repeat some of this data by jumping the height above the ground another five or ten feet, to see if you get the same thing.

MR. GUSTAFSON: Well, the wind velocities can do a similar thing.

MR. BORST: Yes, that is right. The wind velocity was a very critical item. As I mentioned during my talk, the wind velocity effects, I think, can be minimized by a proper application of constant speed propeller rather than a constant blade angle propeller. Because, when you get a change in wind velocity, you are generating a change in angle of attack, which is influencing the results; and, this is more important than the skew change you get from a five- or ten-mile-an-hour wind.

QUESTION: Frank [Franklyn J.] Davenport, Boeing-Vertol.

I wonder if, in considering this question of the ground, we aren't overlooking the fact that we mostly design tilt wing and tilt prop airplanes symmetrically; that we're actually going to be operating with a plane of symmetry. So, maybe we should put it closer and make like it's what we're trying to test.

Another thing is the question of the blockage behind. I haven't yet seen a successful propeller on an airplane that didn't have a nacelle behind it, and similarly, in most of these designs, we have your wing with its download . . . of course, your X-19 didn't have an engine out there; I guess I have to retract that — but most aircraft usually have the engine right behind the prop. Besides that, there is always a wing right behind it in the designs we're considering; that's going to change not only blockage effects but [also] slipstream rotation. Once Mr. Erickson has sorted out his vortex structure, what is he going to do when there's a wing there to foul it up again. So, I just throw these out as comments about the difficulties that still lie before us and the philosophy that we might do our testing with.

MR. BORST: I would like to make one comment. The reason I think you want to do without blockage, is that you have to have a base point. Then you can add in the blockage, and be only too happy to do that.

On slipstream rotation in the effect, if you remember some of the old wing data on conventional propellers, the improvement in efficiency was quite marked in some of the climb conditions with the wing placed in back. As I remember it, it was like 2 percent if you had a propeller with a high induced loss.

QUESTION: Bill [William C.] Schoolfield, LTV Aeronautics Division.

I want to explore the blockage a little further, also. In general, you seem to get an improvement in figure of merit with the blockage. Have you measured the net force on the whole system including the wing? You touched on that, but I am not sure I quite understood your point.

MR. BORST: Well, I will try to clarify it. Yes, of course, with the X-19, we tested the whole system. We found that the blockage — and this was the number we predicted incidentally — on the rear wing was something like 11 percent, compared with the free air propeller — that you get an 11 percent loss in thrust compared with a free air propeller.

Now, the blockage as measured on that wing is higher than that value, because the propeller is showing an increase in figure of merit due to the blockage. But, what you have to do is start out with the free propeller, add in the effect of the blockage on the performance of the free propeller, and then subtract out the blockage of the wing, or the drag of the wing, so that you get a net result.

MR. SCHOOLFIELD: Was that net result higher or lower than the bare propeller? Do you know?

MR. BORST: The total thrust of the propeller-wing combination was 11 percent lower than the bare propeller.

MR. SCHOOLFIELD: That was the sum?

MR. BORST: That is the sum.

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

I just want to make one comment regarding the use of test data as we have been obtaining it down through the years, and that is — I noticed this in the paper which was given from Therm also — we tend to say that the agreement between the predicted thrust and power looks good — as the comment was made in that paper — and then we say that we missed the shape of the figure of merit curve by a sizeable amount. This doesn't really add up. What we are really saying is that we don't recognize that small changes in the thrust which [are noted] in the power make very large changes in the figure of merit variations. So, in using these data, we need to be extremely careful that we have good experimental results so [that], when we're talking even sometimes 1 percent results, we still can fool ourselves badly.

I want to ask you this question. You mentioned the 40 by 80 data, and I didn't see the comparison in here, and what this meant in terms of Wright-Field, and so forth. It seemed to me that the results we obtained in the 40- by 80-foot wind tunnel were quite adequate to what would be indicated in the flight case. This type of testing then, on this rig, would indicate that such results can be obtained on the static test rig.

MR. BORST: Yes, I agree. At least on the X-100 propeller, that was a pretty close correlation.

CHAIRMAN JACKSON

Up in Canada, across the border, there is an aircraft company which has shown one of the longest continuing interests in propeller-driven VTOL aircraft I think of any of them. This, of course, is Canadair. They have been active back through, I guess, the late 1950s and early 1960s; [they] proposed very actively on the tri-service competition back in 1941 and 1942, and more recently [they] have been flying, quite successfully, a tilt-wing aircraft designated the CL-84.

This aircraft does use a foreign propeller system (I might hasten to add), constructed down in New Jersey, but, it also has performed rather well. I think the success of the propeller system (although I am not completely familiar with the details) is undoubtedly due to the work performed by Hank Borst; but, also, Canadair has shown a very aggressive interest in research and propeller system work themselves and [they have] conducted much experimental testing and analysis in this area.

To tell us about some of this activity, now, will be Dr. Glenn Adams from Canadair. [The following is] just a brief sketch of his background.

He was born in Montreal in 1928. [He] attended McGill University, obtaining a Bachelor of Science in Mathematics and Physics in 1949, a Master of Science in 1950, and a Doctor of Philosophy degree in 1953.

He spent three years in the Helicopter Design Office of Bristol Aircraft Limited in England, followed by one year with Bristol Aircraft Limited in Winnipeg. Since 1957, he has been engaged in VTOL Research and Development at Canadair.

He is a member of the Canadian Aeronautics and Space Institute, American Helicopter Society, and Corporation of Engineers of Quebec. [He is an] Associate of the Wingfoot Lighter-Than-Air Society [and an] Associate Fellow of the Royal Aeronautical Society.

PROPELLER RESEARCH AT CANADAIR LIMITED

by

G.N. ADAMS

Canadair Limited

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN JACKSON: That was a fascinating sequence, Glenn. We are running a little short of time. I think we are going to have to limit this to just a very few questions, unfortunately, for this paper. So, only ask a question if you really think you have something that is real hot.

QUESTION: Jan [M.] Drees, Bell Helicopter Company.

I am particularly interested in your comment about the autorotation of the outward point of the blade, and I wonder if this is concluded from the average flow pictures or if this is indeed true for the discontinuous flow situation when the blade forms the tip vortex. That might make quite a difference, I believe. Would you comment on that?

MR. ADAMS: Well, the only flow measurements we have done so far have been average values. However, from the smoke pattern at the moment that the blade passes through the smoke, I think one can see that the flow does have a forward component, at least at the very outer tip of the blade. So, I would think that the instantaneous measurements should show this also.

I suspect that it takes a very little forward velocity to get rid of this, and I would tend to disagree with Hank Borst's suggestion about using constant speed propeller to cope with the effects of the wind. I would say if there is any wind blowing, you are not measuring the static condition.

MR. DREES: That was not the impression I got from the flow pictures where the tip vortex was just forming. It looked to me that you indeed had a positive flow rearward towards (. . . downward through) the propeller.

Now, that brings up the next question actually. If you have a lot of twists at the tip, you may have overtwisted it there and get a negative angle of attack?

MR. ADAMS: I am sure that the twist distribution near the tip of the blade is very critical, and on the propellers we've tested so far, it is not right.

MR. DREES: Thank you.

QUESTION: [Paul F. Yaggy, Army Aeronautical Activity.]

I just want to ask specifically [what] the relative dimensions between the propeller and the chamber in which you are doing this testing [are], the approximate length of time that you feel you can operate without developing recirculation, and what means you have taken to determine when you do have a positive in-flow into the propeller?

MR. ADAMS: Well, the propeller diameters we are talking about now are 4 or 5 feet, and the propeller axis is about 8 feet above the floor, which is the closest boundary. The room which we are in at the moment is 130 feet square and has a lot of surface tooling stored around the outside of it, which helps to break up the recirculation a bit. The ceiling is about 35 feet high. We feel that . . . well, obviously, the circulation is going to build up as soon as you start running the propeller, and this shows up in fluctuations in thrust and torque on the instrumentation. We have a remote-reading temperature probe upstream of the propeller which responds very rapidly. We haven't, so far, found much in the way of temperature fluctuations up there.

I would rather feel that, after you have been running for a number of minutes, you can build up a more or less steady condition in which you certainly have some recirculation effect but, hopefully, not too large.

Now, in the work that we are doing, we are primarily concerned with comparing the performance of different designs of propeller blades. Obviously, there is going to be some scale effect between the model blade and the full scale ones; there may also be effects due to the environment in which we are testing. We are hoping that these will not obscure the comparisons between one blade design and another, even though the actual level of performance which we measure may be off by 1 or 2 percent.

QUESTION: Don [D.N.] Meyers, Piasecki Aircraft Corporation.

I just wondered whether you have any theory — or hypothesis of any sort — as to why only every other vortex seems to have a tail?

MR. ADAMS: I think this tail is probably part of the vortex sheet which is moving downstream at the slipstream velocity, and it sort of attaches itself to each successive vortex for awhile as it goes past. This is sort of a crude interpretation, but this is the way we've looked at it so far. Valensi's analysis [Reference presented on page 9 in prepared paper] shows that the velocities of the air at various stations along each of these curls is quite different, and, at the same time that all of this stuff is going downstream, you tend to get the flow moving in towards the vortices — the flow is feeding the vortex. This would also tend to make the inboard arm of the tail move downstream faster than the core does.

CHAIRMAN JACKSON

We are coming to the final paper [of the morning session] now. We have covered so far in the first three papers pretty much the story on isolated propeller performance problems. The next paper is going to deal in some degree and in some depth with the effects of aircraft bodies in back of the propeller and the effects on propeller performance, and will continue this into a discussion of loading and possible structural problems on the propeller.

Andrew Trenka will present this paper. He received his Bachelor of Science in Aeronautical Engineering from Purdue University in 1956 and his Masters from Cornell University in 1961. He has been working at the Cornell Aeronautical Laboratory from 1961 to the present.

His professional experience has been in the fields of aerodynamics, aeroelasticity, flutter, heat transfer, engineering mechanics, and orbital mechanics. He has been project engineer for an investigation of the dynamics of a helicopter jet-flap rotor system, an optimization study of a heat-rejection system for space vehicles, and an investigation of VTOL propeller performance.

PREDICTION OF THE PERFORMANCE AND STRESS CHARACTERISTICS OF VTOL PROPELLERS

by

A. R. TRENKA

Cornell Aeronautical Laboratory, Inc.

[Paper contained in Volume I of the proceedings]

QUESTION AND ANSWER PERIOD

QUESTION: Andy [A. Z.] Lemnios, Kaman Aircraft Corporation

There are a couple of questions I have regarding your analysis. I understand that you used the method of Houboult and Brooks and [that you] determined your aerodynamic forces independently and then used those as aerodynamic excitation to get the forced equations of motion. How did you account for aerodynamic damping in the method of Houboult and Brooks, because they don't have any aerodynamic damping in their equations?

MR. TRENKA: In each mode, we assumed that we had a damping term which was proportional to the displacement and in phase with the velocity — in terms of the uncoupled modes.

MR. LEMNIOS: Did you have any trouble achieving convergence?

MR. TRENKA: In the overall method?

MR. LEMNIOS: Yes?

MR. TRENKA: No. Benefiting from Mr. Piziali's experiences with his program, we inserted a pseudo-quasi steady damping term on both sides of the equation, which took out all of our convergence problems — we had no convergence problems at all.

MR. LEMNIOS: Was this damping term in both bending and in torsion?

MR. TRENKA: Yes. It also had the coupling terms in there.

MR. LEMNIOS: Okay. There are a couple of other questions in this same regard. Do you account also for any variable inertia distributions in the propeller itself. Can you account for variable inertia or variable chord geometry?

MR. TRENKA: Do you mean - I don't understand - varying with azimuth?

MR. LEMNIOS: No, varying radially.

MR. TRENKA: We have a continuous distribution; the mass distribution is accounted for.

MR. LEMNIOS: Okay. But it is not necessarily uniform?

MR. TRENKA: No, it is definitely not uniform.

MR. LEMNIOS: One other question. In calculating the inplane stresses, did you incorporate any coriolis forces?

MR. TRENKA: No.

MR. LEMNIOS: Due to out of plane motion?

MR. TRENKA: I beg your pardon. Yes, there was, but subject to some restrictive assumptions, the details of which I don't remember. But, I do remember that the coriolis effects were included in the determination of the longitudinal stresses.

MR. LEMNIOS: Thank you.

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

Maybe I missed it, I am not quite sure. You have been assuming a translational velocity of the aircraft in this work?

MR. TRENKA: Yes. There are no trim equations in the work as we stand now. We assume a given flight condition, and proceed from there.

MR. YAGGY: I see. Now, all of these forces as a function of τ that you have been presenting have been non-dimensionalized — or they have been normalized, I should say — by some arbitrary assumption, I guess?

MR. TRENKA: Yes, that is right.

MR. YAGGY: Have you by any chance looked back at NACA-TR-1295 to see how this work is checking out with the things that were measured at that time?

MR. TRENKA: On what ship?

MR. YAGGY: This was not on a ship; it was on an isolated propeller.

MR. TRENKA: No. These results are for a specific blade configuration and didn't match any of the data published to date.

MR. YAGGY: Are you planning to go back and see how this would work out?

MR. TRENKA: Yes. Well, right now, we are involved in an experimental program where we hope to obtain the measured performance and blade stress data on an isolated propeller. The propeller which we are going to be using is the 0.6 scale model of the XC-142 — the one that was tested in the Ames Tunnel.

MR. YAGGY: What I wanted to point out is that, in TR-1295, there are three propellers that are already run through a very sizable range and quite a wide variation of parameters, and also there is some work by McNamara [which] exists earlier than that on a fourth propeller.

MR. TRENKA: Yes, I am aware of most of these experimental results; but, there again, number one, there is some question about the accuracy of the results and, number two, in order to run studies on these propellers, quite a large reprogramming of the aerodynamic characteristics of the blade is required.

MR. YAGGY: The other question that I had [concerned] the variations with τ in those particular areas — I wonder how much the normalizing has an effect on this — showed a much more gradual rise, particularly in the thrust up until say around 70 degrees of pitch and then a very rapid increase.

MR. TRENKA: The normalizing effect has distorted the curves somewhat. The actual values would have shown on a somewhat steeper inclination.

QUESTION: [Speaker did not identify himself]

Andy, did you use steady-state, two-dimensional airflow data in all of your analyses?

MR. TRENKA: I did.

MR.__________: Do you have any idea what might happen if you were to use nonsteady aerodynamics?

MR. TRENKA: Not the vaguest.

CHAIRMAN JACKSON: That brings us to the end of this session, and I want to thank all of the authors for an outstanding group of papers.

TECHNICAL SESSION II
WEDNESDAY AFTERNOON, 22 JUNE 1966

SESSION CHAIRMAN
F. J. DAVENPORT
Vertol Division of The Boeing Company

Before we begin the actual papers, I want to add one more or less personal anecdote.

Three years ago, AVLABS — then TRECOM — and Cornell held the Dynamic Airload Symposium, and this was just a week or two after I had joined the helicopter industry. I had come out from the Airplane Division of The Boeing Company in the north woods and, at that time, I got my first introduction to the magnificent complexity of helicopter aerodynamics.

After the well behaved solutions that you could get for fixed wing aerodynamic problems — things like that — it was a real eye-opener. My experience since then has not been the least bit disappointing . . . the complexities do not go away.

I hope these symposia that AVLABS and Cornell have been having will turn into sort of a permanent tradition. I will be looking forward to another one in a couple of years. I trust we will have problems to talk about then.

Now, the first paper to be given is the Performance Potential of Rotor Blade Inboard Aerodynamic Devices by Dr. Maurice Young and Jaan Liiva, both of the Vertol Division of The Boeing Company.

Dr. Young is Manager of Advanced Technology at the Vertol Division of Boeing. He holds the Doctor of Philosophy degree in Engineering Mechanics from the University of Pennsylvania, a Master's degree in Mathematics from Boston University, and Bachelor's degrees in both the Physical Sciences and Liberal Arts from the University of Chicago.

His professional experience of more than fifteen years in Aeronautical and Aerospace Engineering includes over a decade of helicopter and V/STOL engineering research and development, as well as missile, satellite, and avionics work. Dr. Young came to Boeing from the Philco Communications and Weapons Division where he was Manager of Applied Mechanics with principal emphasis on aerothermodynamics, flight mechanics, and structural dynamics. Since he joined The Boeing Company in 1961, his activities have centered on advanced helicopter systems research and on advancing the supporting technologies of helicopter aerodynamics, dynamics, and structures.

Dr. Young is a member of the Society of Sigma Xi, an Associate Fellow of the American Institute of Aeronautics and Astronautics, and a member of the American Helicopter Society. He is a member of the Society's Committee on Helicopter and V/STOL Design, and a member of the AIAA Technical Committee on Structural Dynamics. He has published, as I am sure you are all aware, a number of papers in the helicopter and applied mechanics fields.

Jaan Liiva, who will present the paper, is Group Leader for Rotor Aerodynamics in the Aerodynamics Staff, Vertol Division of The Boeing Company.

He was graduated from the University of Toronto in 1960 with a Bachelor of Science degree in Engineering Physics. He attained a Master of Science degree in Aeronautical Engineering in 1961, and is now actively engaged in a program leading to a Doctorate in Engineering Mechanics at the University of Pennsylvania. He joined the DeHavilland Company of Canada as an Aerodynamicist in 1961, where his work concerned stability and control of STOL aircraft.

In 1962, he joined Vertol and worked both on stability and control and performance problems. In 1964, he was transferred to the Aerodynamics Research Unit, where his present responsibility is theoretical and experimental work associated with rotor aerodynamics. Mr. Liiva is a member of the Phi Epsilon Alpha, McGill University Engineering Honorary Society.

PERFORMANCE POTENTIAL OF ROTOR BLADE INBOARD AERODYNAMIC DEVICES

by

M.I. YOUNG and J. LIIVA
Vertol Division of The Boeing Company

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN DAVENPORT: When asking questions, I would like you to state your name and affiliation even if you have already been recognized in previous questions and so forth because that way the recording will be tagged so that we will have the right speaker down in the recorded minutes of these discussion sessions. There will be people in the aisles with microphones; please go to them. The floor is now open to questions.

QUESTION: [A. C.] Adler, Hughes Tool Company.

Nowhere in the paper do you state what the effect of L/D would be for a typical helicopter, what the power loading would be for a helicopter operating at your μ of 0.6. Do you have that number at your fingertips, perhaps?

MR. LIIVA: Are you talking about the power required for blowing?

MR. ADLER: No, I am talking about, let's say, the segmented rotor.

MR. LIIVA: That would again depend on how much the advancing tip mach number is or . . .

MR. ADLER: [Interrupting:] Well, let's say for a typical case where you might go to 0.9 on the advancing tip mach number?

MR. LIIVA: I don't think I have an answer for that.

MR. ADLER: Well, let's say I've got a number based on your low mach number on the model. Now, if the model tests out of your figure 11, it comes out to an L/D of 2.2 and a power loading of 3. I don't know how well that compares with a compound?

MR. YOUNG: I am sorry to say, Abe, that I don't recall the overall lift-drag ratio. I can say with certainty though that we were constantly comparing this approach with one using a propeller; and, during this program, we coined the phrase "thrusting rotor" or "compound without a propeller." So, apart from the mechanical complications which Jaan eluded to, the overall lift-drag ratio of this type of rotor — providing all the propulsive force and providing a fraction of the lifting force — in other words, a partial lift on loading — was higher than any compound system that we are able to conceive using a conventional propeller. I have just received a signal from one of my colleagues whose memory is better than mine, and he indicates 7; [Directing comment to Mr. F. D. Harris:] is that right, Frank?

MR. HARRIS: Yes, the comparison of the data that are presented in the paper are made with a model hub included in the L/D. I am sure you can [remove] that model hub as most model hubs are more drag than you might get full scale. If you remove the hub, you will find that the L/D is an order of magnitude of 7, and the advance ratio is 0.6. [With] a disloading quarter magnitude 8, running at a constant forward speed of 220 knots and a compound motor running in its optimum L/D, and a twist like 2 or 3 degrees, you'd probably achieve an L/D of 7 or 8. I think that, looking at your L/D, the rotor system is quite comparable.

QUESTION: Bob [Robert A.] Piper, AVLABS.

Just a note of historical interest; I believe the concept of an elliptical airfoil with blowing out the leading and trailing edge for just this purpose was patented by Von Karmen [?]and a fellow named Professor Wan [?] back in the mid-1940's. Further, under AVLABS contract, Professor Wan [?], who is now at the University of Texas, has been running some wind-tunnel tests on an elliptical airfoil section to see what the influence is of blowing upstream and to see what the possibilities are of getting the kind of frequency response out of the blowing that you would need in application to a helicopter rotor. It seems to me that final report should be in preparation one of these days.

One of our worries has been that you have to get the air for a jet-flap system from somewhere — and this would be a compressor somewhere in the fuselage — and blow this air up through the hub and duct it out to the blade. I was wondering if you had run any preliminary design studies or calculations to see (a) how this would, in fact, be mechanized, and (b) how much power you realistically need in this system to include in your power comparisons?

MR. LIIVA: The answer to the first question is that we have not made any design studies as of yet.

Now, the types of numbers that I was looking at in this last slide were like 500 or 600 — between 400 and 600 horsepower for a three-bladed rotor to compress and to pipe it up. I am talking here about a 30-foot radius rotor. This is a first guess and, of course, as time goes by, it may change in either direction.

QUESTION: John [L.] McCloud, Ames Research Center.

I would like to comment — first, if you consider the inboard cyclic to be a relative proposition, then your concept here is quite similar to the Durand jet-flap rotor which has the cyclic going on outboard, and the inboard side does not — being cycled only by way of flapping, feathering equivalents. I would like to ask a question [with] two points in regard to the scheduling of the inboard pitch.

Number one, just how did you get such unusual harmonic content mechanically; and number two, did you investigate just putting in the first harmonic, in your theory at least?

MR. LIIVA: Yes, I did. Let me qualify this — by the first harmonic, do you mean a shifted first harmonic or . . .

MR. McCLOUD: Yes, anything you could do with a mechanical torque twist as contrasted with cams and so on.

MR. LIIVA: Oh, I see. No, I have not looked at that. What I have looked at is a fixed, step-wise change in, let's say, angle of attack between the inboard and outboard — in other words, keeping the inboard fixed at one angle of attack and keeping the outboard fixed at another angle of attack.

MR. McCLOUD: How did you do it? How did this model work?

MR. LIIVA: We made it work by having a cam, essentially, or a round tube with a cam follower.

MR. McCLOUD: A warped swash plate?

MR. LIIVA: No, not a warped swash plate. [Directing comment to Mr. M.I. Young:] Maybe you can explain it, Maurie.

MR. YOUNG: Just to comment quickly on that whole series of questions. We have investigated the simple first harmonic. I think there is a prior study on this; I believe Jan Drees might comment. This was run by Bell with an antiphased pitch schedule.

Actually the idea was to try to optimize the section lift-drag ratio at each radial and azimuthal station, and the original concept was for a twistable segment, which you can imagine would be more complicated than a rigid one.

So, the rigid segment was a compromise on the radial angle of attack distribution, but we made as little compromise as mechanically possible using a cam system on the azimuthal variation. As you saw in the data, a simplified schedule — which was still pulsatile rather than sinusoidal — returned about 80 to 90 percent of the potential gain with a very complicated schedule.

I think one point that's made in the paper that may not have come through too strongly is the idea that it is a pulse type of pitch change as opposed to anything that you would approximate with a Fourier series, unless you took an infinite number of terms.

QUESTION: Jan Drees, Bell Helicopter Company.

We at Bell studied this problem several years ago, and it was published in 1963. There we investigated a feathering cuff where we could make this step-wise constant change in angle of attack and first harmonic motion independent of the blade feathering, and we discovered that there are two ways to feather this cuff to get optimum results. The first way is the one you are using here in your study where, on the retreating side, the blade in the reversed flow is lined up such that the air still comes from behind and the trailing edge [is] first.

The other way of feathering the cuff is applicable to rotors which are tilted farther over than the ones you studied here and [is] where you try to keep the nose of the airfoil and the cuff into the wind and keep low drag all of the way. The improvements we got, theoretically, were on the order of 10 percent power saving, and we got a considerable reduction in oscillatory loads.

Now, the 10 percent reduction is lower, I believe, than the savings you show here. I wonder if this is due to the fact that you compare the performance only to a zero twist rotor which, of course, at high speeds we have to tilt over several degrees — at least 10 degrees, say. A zero twist rotor will be rather poor from a propulsive efficiency [standpoint]. You have to put some twist in, and the farther over you tilt the rotor, the more twist you need, so that the comparison, I think, is a little unfair with a rigid rotor type without a feathering cuff. I think you should get slightly better propulsive efficiency.

MR. LIIVA: We looked at a twisted rotor also. In this particular case I investigated a rotor with 6 and 9 degrees of twist. This was done theoretically. I did not see any difference in the performance at this particular tip mach number or tip speed ratio of a twisted rotor.

In other words, it did not improve the propulsive efficiency or the power required. This may be contrary to . . .

MR. DREES: [Interrupting:] Yes. It certainly is. As a matter of fact, the way you feather it off the cuff on the advancing side, you have about 30 degrees angle of attack. And, that is quite an important area . . . at these speeds.

MR. LIIVA: Yes. Maurie just indicated to me that we have nonuniform downwash in this calculation. This would make quite a difference in the calculations, that is, especially when you have very high lifts and have very sharp gradients in lift. It is only by having nonuniform downwash that you are able to obtain a reasonable distribution of induced angle of attack or induced lift.

CHAIRMAN DAVENPORT: [Recognizing question from the floor:] This will probably have to be the last one, but we will see how long this question is.

QUESTION: Norman Gorenberg, Lockheed-California Company.

There are several items that come to mind in the presentation of this subject. I think it is certainly desirable and important to examine what may be done further with the basic rotor system on the helicopter. I think there are several points that come to mind also in the exploration of this kind of work. At least I have found that the inboard area has been examined in at least three places and published in two [reports] besides the ones that are discussed regarding the boundary layer control — and that is the one that Mr. Drees wrote and the other one in the early 1963 period, I believe, published by Mr. Sheerer [?] of Lockheed. He had examined a multi-segmented blade in the same process.

Along this whole approach, however, when you have the segmented sections, I think you also have to consider the spanwise flow near the hub as the segments pass through the regions of 180 degrees and zero degrees. I didn't perceive, from the comments made in the presentation or in the paper, exactly where the test and the theory tied together or were done separately. I don't quite understand that at this point.

Also, in considering compounding, I think various investigators have examined both approaches and perhaps, with a mechanical requirement at particular stages in the development of the vehicle, have found that the compounding was more readily accessible at this time — especially in light of what has to go on in the proximity of the hub and in hub drag. I didn't get from the comment that Frank Harris made as to whether the hub drag was subtracted in both cases. It would be very nice if we could take it away in both cases, but it is always there. We end up in an area of design which depends very much upon the details — they are no longer details. I think you have to examine both systems more fully in this area. [That is the] end of [my] comment.

MR. LIIVA: Yes, I agree that, to be able to evaluate both systems — mechanical and blowing — you have to do essentially a design study, and look at the weight aspects of it, and look at the drag aspects of it, and look at all of the aspects.

All I have done here is [I have] shown that a mechanical device on the inboard area of a rotor can produce the type of propulsive forces that are sufficient to propel an aircraft at the speeds we are talking about. And, secondly, [I have shown] that you can obtain similar types of propulsive forces by using, say, blowing.

Now, as far as implementing these and coming up with a design — that, of course, is in the future.

CHAIRMAN DAVENPORT: Mr. Lichten, I think we just have time for your question.

QUESTION: [Robert L. Lichten, Bell Helicopter Company.]

I would like to congratulate the authors on a fine study here. I do note that, in Figure 3, they have introduced a concept of propulsive efficiency and have repeated that term in some of the later figures without giving the reference line which would allow its computation. I just made a quick note on it and it looks to me that, at μ of 0.7 — which most of the figures cover — the resulting propulsive efficiency, using the authors' definition, would still be, I believe, down somewhere around 0.5. I wonder if they have checked this number and if this is the case?

MR. LIIVA: Yes. As you get to higher and higher propulsive force requirements, we have generally taken our calculations to way beyond the normal applicability to a helicopter, and the upper end, in general, will be quite low, because the line just starts curving over no mater what μ you compute at . . . even at 0.3.

CHAIRMAN DAVENPORT: Thank you very much, Jaan.

CHAIRMAN DAVENPORT

The next paper is on the subject of Aerodynamic Loading of High-Speed Rotors. The authors are Mr. John P. Rabbott, Jr. and Mr. Vincent M. Paglino of Sikorsky Aircraft.

Mr. Rabbott is now Assistant Supervisor of the Aircraft Advanced Research Section at Sikorsky Aircraft. He received his B.S.A.E. degree from the Massachusetts Institute of Technology in 1951 and his M.S.A.E. from Rensselaer Polytechnic Institute in 1962.

His professional experience includes periods from 1951 to 1956 as an Aeronautical Research Scientist with NASA, at Langley Research Center, working in the Langley Full-Scale Wind Tunnel. His work at that time concerned rotor aerodynamic research, including measurements of rotor dynamic air-loading and extension of rotor performance theory.

In 1956, he joined the United Aircraft Research Laboratories and conducted analytical studies of novel wing concepts and VTOL propellers incorporating boundary layer control and variable geometry.

Since 1959, he has been with Sikorsky Aircraft and is currently responsible for advanced research in fields of aerodynamics, dynamics, and aeroelasticity for both rotor and non-rotor V/STOL aircraft.

Mr. Paglino, who will give the paper, is a Research Engineer with the Aircraft Advanced Research Section of Sikorsky. He received a B.S.A.E. degree from Rensselaer Polytechnic Institute in 1963 and an M.S.A.E. from West Virginia University in 1964.

In 1964 he was a Research Assistant at West Virginia University, conducting experimental studies of VTOL propeller static performance and transition characteristics.

From 1964 to 1965, he was a Flight Development Engineer for Sikorsky Aircraft. His activities involved the CH-53A control system and flying qualities evaluation and studies of fuselage vibration and response characteristics.

Since 1965, Mr. Paglino has been a Research Engineer in aerodynamics prediction. He is presently involved in studies of high-speed rotor airload prediction and correlation with full-scale tests.

Now, I will turn the microphone over to Mr. Paglino.

AERODYNAMIC LOADING OF HIGH-SPEED ROTORS

by

J. P. RABBOTT, JR. and V. M. PAGLINO

Sikorsky Aircraft

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN DAVENPORT: Thank you very much, Vince. I am going to trespass on my advantage as the Chairman of the session to ask him the first question myself.

I have seen evidence in various places that, on the retreating side of the blade operating anywhere near stall, we can run into normal force coefficients in excess of the two-dimensional steady flow stalled value, and I wonder if you guys had run across that anywhere?

MR. PAGLINO: Yes. The reason I didn't mention it is that we are still studying it and we're not ready to release any definite statements on the matter yet. But, we have observed this.

CHAIRMAN DAVENPORT: I don't think you should be afraid of it, because I think it has happened in plenty of places. O.K. I will give people on the floor a chance. Frank Harris.

QUESTION: Frank [F.D] Harris, Boeing-Vertol.

I would like to compliment Sikorsky and TRECOM in executing this portion of your contract. It is just this kind of quality of work that is going to be required to get us anywhere out further in speed. I had the same question that Frank asked: What was the highest level of — I guess you would call it C_n, normal force coefficient — that you observed in looking through

the data a..., from a broad sense, did this rotor experience blade stall . . . I'd rather have you define blade stall.

MR. PAGLINO: I was just going to ask you to.

MR. HARRIS: I guess . . . something bad. In particular, the third item, have you looked at C_n's that would be available from your chordwise pressure data, and how would that agree with your two-dimensional data so far?

MR. PAGLINO: The first question; I don't remember the exact number on C_n's that we have gotten — I think we have gotten them as high as 1.5 or so . . . 1.6, something on that order. Your second question was?. . . I'm sorry . . .

MR. HARRIS: Well, blade stall . . . if [that term is]admissible.

MR. PAGLINO: I wasn't present during the test. [Directing question to Mr. Rabbott:] Did you observe [it] in the tunnel? [To Mr. Harris:]I will get my co-author in here.

MR. RABBOTT: We had one condition in the tunnel which hasn't been presented here, because we thought at first that the data wasn't correct. We found out, however, that in the data reduction process of the performance data, there was an error made which has since been corrected; and — this is at 110 knots — we did get into a condition of very severe blade stall. The lift was about 15,000 pounds [with]a couple-thousand pounds of propulsive force. This was using the NASA performance charts which we turned out; this was off the charts as far as our stall limit goes. Looking at the blade stress-time histories, there is a very, very large — 7 per rev[olution] — torsion stress, which is the blade natural frequency, and this also feeds back into the air loads. We hope to present this as an AHS paper this year.

MR. PAGLINO: You had a third item?

MR. HARRIS: Pitching moment?

MR. PAGLINO: No. We were a little bit hesitant to calculate pitching moment on the basis of the number of pressure taps we had.

QUESTION: [A. C. Adler, Hughes Tool Company.]

Just one more question. Are there any prospects of getting the pressure distributions at the high mach number at any time in the near future?

CHAIRMAN DAVENPORT: Jack Rabbott will answer that one.

MR. RABBOTT: The highest mach number we got during this program was 0.84, I think, which is for the 175-knot case shown here. We have a flight-test program coming up on the S-61F — our research compound helicopter — which will have a pressure-instrumented blade and this will go to higher mach numbers . . . well above 0.9.

QUESTION: Andy [A. Z.] Lemnios, Kaman Aircraft Company.

I notice that, at the 85 percent radial station, you have 11 chordwise stations for measuring your pressure distribution; were the pressure gauges here surface-mounted or were they buried? And, if they were buried, did you notice any significant change in bending stiffness or torsional stiffness locally. And, what effect did this have, if any, on the outboard motion of the blade itself?

MR. PAGLINO: I lost you. Which . . .

MR. LEMNIOS: [Interrupting:] Well, at the 85 percent radial station you have 11 pressure transducers . . .

MR. PAGLINO: [Interrupting:] Which figure is this?

MR. LEMNIOS: Figure 2, [where]you are just listing all of the stations. At 85 percent radius, you have 11 pressure transducers buried in the blade, it seems to me you have to remove quite a bit of material, which in turn would reduce the stiffness locally.

MR. PAGLINO: Yes, we took this into account when we inserted the blade's physical properties into our theoretical model.

MR. LEMNIOS: Did this have any significant change in the outboard motion of the blade?

MR. PAGLINO: Not as much as you might expect. No.

MR. LEMNIOS: Thank you.

QUESTION: [M.I. Young, Boeing-Vertol.]

I have two questions. As a follow-up to Jack's comments on seeing a 7 per rev[olution] oscillation in torsion . . . this suggests the presence of stall flutter; I'd like to have your comments on that. Also, in the slide in which you compared the angle of attack distributions as calculated by uniform and non-uniform downwash, I noted that the non-uniform downwash constant angle of attack contours didn't close. Of course, one of the benefits of a three-dimensional vortex theory would be to impose the true boundary conditions, so I would have expected to see a zero angle of attack on the periphery and the closing of all of the other angle of attack contours.

MR. PAGLINO: [Comment directed to Mr. Rabbott.] Do you want to take the first question, Jack, on the stall flutter?

MR. RABBOTT: Yes. That may be a case of stall flutter, and that is what we are looking at now. The reason we are just looking at the data right now is that we only recently . . . as I mentioned before, we thought that performance data was in error.

So, we didn't reduce the airloads and stresses until recently. Once we saw the results, we got much more interested, and we are now doing a theoretical study of stall flutter under AVLABS contract. We intend to apply these results, which are based on oscillating airflow tests, in the analysis of this airloads data. We hope this will come out as part of that [AHS] paper.

MR. PAGLINO: As far as the angle of attack contours . . . you were right, it would have been more advantageous to have them close. At the inboard stations, we couldn't compute the angle of attack contours due to a limitation in our program which we are correcting. The way we compute the response of the blade . . . there is a sign reversal in angle of attack — in the sign convention of angle of attack in the reverse flow region — and when we try to do a harmonic analysis through this region, we get some pretty funny looking things; they are meaningless because of a change in sign convention. So, that is why we don't have them in there.

CHAIRMAN DAVENPORT: We have time for one and maybe two more.

QUESTION: John [L.] McCloud, Ames Research Center.

I am fairly certain that you have not incorporated anything like a tunnel wall correction in your data here, and I would like to speak particularly about its effect on variable in-flow. Be careful in modifying your wake [so that] you don't have a wake of a rotor in the wind tunnel or, when you do get it, save it for us.

Secondly, with good agreement in the first four harmonics, I wonder if you want to go any further in that regard? Can you comment on this variable in-flow and the effect of tunnel walls?

MR. PAGLINO: You are right, we did not apply any correction for the tunnel wall effect, [because] we had so many other things to do.

MR. McCLOUD: My comment [is] that the area ratio involved here is quite large. You [have] about 2500 square feet of rotor disk area and only 2800 square feet of tunnel area; luckily, they aren't in the same plane or we wouldn't make it.

CHAIRMAN DAVENPORT: Do you have any question or something to add, Jack?

MR. RABBOTT: John, we did look at the overall wall correction to rotor angle of attack based on Harry Heyson's method, and the maximum change in rotor angle of attack would be less than one degree at the worst condition. What the distribution is over the disk . . . I have no idea.

MR. McCLOUD: In the manner you presented the data, the one degree wouldn't show anyway.

MR. RABBOTT: No.

CHAIRMAN DAVENPORT: Bob Loewy has a question.

QUESTION: [Robert G. Loewy, U.S.A.F.]

Actually a comment, Frank. This seems an appropriate place to mention — with regard to the very high C_n question which I think Frank Harris raised — [that] sometime back, as part of a rotor noise study that was done for John Yeates at AVLABS, we reviewed the H-34 pressure data that was done at Langley and also the Bell data and found that, in calculating C_n's around the disk for various flight conditions, the C_n's exceeded what would be expected from two-dimensional wind tunnel results. We suspected at that time that the inplane components of the variable induced velocity around the disk were responsible, and so we made (and I think you may remember) a rough check to see whether this could be the cause. In fact, we found that those components were, if anything, in the wrong direction. So, having these very high C_n's could be interpreted (as far as I could tell) only in terms of differences from three-dimensional and steady effects.

MR. PAGLINO: [That] may be.

CHAIRMAN DAVENPORT: I would like to ask another question myself. In the dynamic pressure that you used to get your C_n's, did you include any estimated value of inplane induced velocity? We found that that makes quite a noticeable difference in your computed airloads, and I just wondered if you had noted the same thing?

MR. PAGLINO: No, we didn't use any inplane components. Did you say induced velocity?

CHAIRMAN DAVENPORT: Right.

MR. PAGLINO: No, we have not tried incorporating those yet. This is something we may do in the future.

CHAIRMAN DAVENPORT: Do we have any more questions from the floor? George Kurylowich.

QUESTION: [George Kurylowich, The Boeing Company]

In Figure 7, you show a phase shift between theory and experiment.

Now, is this due to vortex distortion, do you think, and why does it persist when the forward speed increases. You would think the distorted vortices would be farther away and the phase shift would reduce.

MR. PAGLINO: This is another area under examination. We commented a little bit on it in our principal test report. We don't believe it is an instrumentation distortion. We did some analysis of the acquisition and reduction system, and we found that it was an overall amplitude phase lag of only 5 degrees at the 20th harmonic and much less than that at the lower harmonics. It appears that it is an aerodynamic effect or something lacking in our calculation of the structural response of the rotor.

MR. KURYLOWICH: Thank you.

CHAIRMAN DAVENPORT: Well, thank you very much, Vince, and also thanks to you, Jack Rabbott. We would like to reconvene promptly at 3:45, because these papers are generating a lot of discussion, and we would like to have time for all [of them].

CHAIRMAN DAVENPORT

The next paper is entitled "Prediction of Rotor Wake Flows." It is to be given by Dr. Peter Crimi of Rochester Applied Science Associates. Mr. Crimi received a Bachelors Degree in Mechanical Engineering from Cornell in 1958, a Masters in Aeronautical Engineering in 1959, also from Cornell, and a Ph.D. in 1964 from Cornell.

From 1959 through 1966, Dr. Crimi has been with Cornell Aeronautical Laboratory, one of our hosts today. His work has been in a number of fields including artillery rocket control systems, computation of aerodynamic loads on helicopter blades, studies of flutter characteristics of jet-flap rotors, investigations of forces and moments on oscillating hydrofoils and on hydrofoil flutter problems, and the theoretical study of helicopter aerodynamics.

He has only recently joined the Rochester Applied Sciences Associates and, if I am not mistaken, the work he is reporting was done while he was at Cornell Aeronautical Laboratory, Inc. I will turn the meeting over to Dr. Crimi at this time.

PREDICTION OF ROTOR WAKE FLOWS

by

P. CRIMI

Rochester Applied Science Associates, Inc.

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN DAVENPORT: Thank you very much, Peter. The floor is now open for questions.

QUESTION: Harry [H.] Heyson, NASA Langley Research Center.

First off, Peter, I would like to congratulate you on the formulation of a real messy problem. You've certainly got some of the major features of the flow in here, including the roll-up of the wake behind the rotor. On the other hand, I would like to take exception on one or two points. In the flow surveys of the TR-1319, the first-order effects of the body were removed by surveying behind the apparatus with the hub turning but no blades, and subtracting these from the measured values with the blades on.

Now, there are admittedly second-order effects due to the flow changes over the body.

MR. CRIMI: I see I had missed that in the report.

MR. HEYSON: It is in there . . . in the section entitled "corrections."

The difference that you are seeing near the central portions of the flow below the rotor is most assuredly due to the omission of the radial variation of loading on the rotor. You can get by without this as long as you are looking at regions sufficiently far away from the rotor, as you mentioned earlier, but you are very likely to get into serious trouble when you get very close to the rotor.

MR. CRIMI: This doesn't pretend to duplicate the flow in the rotor plane.

MR. HEYSON: The other comment I have is that you have put in here an azimuth-wise variation of vorticity. Now, associated with this, there is also shed vorticity at the trailing edge of the blade and, at least in terms of time average values, TN D-809 indicates that, for any particular harmonic vorticity, the contribution of the radial vorticity is the same order of magnitude as that of the circumferential vorticity. I think your agreement might be improved a little bit more if you had managed to keep this feature in.

MR. CRIMI: I find that a little hard to believe, since the trailing vorticity must be the same order of magnitude as the circulation about the blades, whereas the shed vorticity is simply the order of magnitude of the change in circulation.

MR. HEYSON: That is right, but the two of them do come out to be the same order of magnitude, and you will find that in TN D-809, for the time average case, the assumption of such rapid roll-up really isn't justified. I think it's been indicated very nicely this morning for the hovering case by some of the flow pictures that we saw.

CHAIRMAN DAVENPORT: Are there any further questions?

QUESTION: Frank [D.] Harris, Vertol Division of Boeing.

It seems that we are getting quite a bit closer to a good understanding of where wake vorticity is occurring. I wonder if you can speak at all on what is happening to induced power of rotors? I noticed on your chart that the mean induced velocity was greater than 1, but I didn't know what your v_0 was that you ratioed it to. Glauert helped us along for quite a few years with a very simple expression for the induced power of rotors, and I would like to know where we are now . . . if somebody knows?

MR. CRIMI: That v_0 was simply a momentum average.

As I mentioned previously, this model does not pretend to duplicate the flow in the rotor plane. So, you would be making a serious error to try and compute the induced drag from these computed flows. However, this should be applicable to a blade-loads program, simply to define the wake envelope and the amount of distortion of the exterior part of the wake. And then, in some way, fit in the interior structure.

MR. HARRIS: At least the data that you showed there indicated that the mean was of the same order of magnitude greater than Glauert's and, in comparison to the test data that you showed, would indicate that the induced drag of the rotor is higher than what Glauert said.

MR. CRIMI: That data was some way away from the rotor plane; this wasn't in the rotor plane.

MR. HARRIS: I see. Fine. Thank you.

CHAIRMAN DAVENPORT: Do I have any other questions from the floor?

QUESTION: [Richard P. White, Jr., Cornell Aeronautical Laboratory, Inc.]

I would just like to make a few comments in reply to Frank Harris' comments. If you go back and look at the AHS presentation made by Frank DuWaldt and Ray Piziali (I think) about two or three years ago [you will find that] they presented some results on some induced power calculations we made using our blade-loads program. The indication was there that the induced power is indeed larger than people had expected.

Cornell is initiating a program now where, with the variable wake geometry and some other modifications, we are going to be working on the induced power aspect of the rotor. This is being sponsored by the Office of Naval Research.

CHAIRMAN DAVENPORT: I'd also like to add the word [directed to Frank Harris of Vertol] that Frank Davenport of Vertol gave some data like that last year . . . at the AHS Forum. Do I have anything else from the floor?

QUESTION: Robin [B.] Gray, Georgia Institute of Technology.

There is a word in the introduction of your written report that is very apt, and I think that is that this is "formidable" . . . it is a very formidable problem. I do have a question: the core radius that you introduced [was] primarily to calculate the geometry of your vortex system — this is correct, isn't it?

MR. CRIMI: Yes, that is right.

MR. GRAY: Now, I notice also in your written report that you say that variations within about 20 or 30 percent in this core radius would not have a large effect on the flow field calculated.

I would agree with this in the wake, and, since this is what your report is on, I have no quarrel with this. I wonder, though, if you would expect the same small variations if you're calculating the loading on the rotor itself? Shouldn't you think that you might have to calculate the core radius to a greater degree of accuracy, particularly if the vortex is close to the rotor?

MR. CRIMI: The core radius really doesn't enter into the wake structure close in to the blade. Hopefully, the roll-up takes place a little ways downstream.

MR. GRAY: Well, the reason I asked [was that] I have some pictures I was going to comment on later, but this may be a good place. Smoke was introduced at the tip of the blade, and the smoke was photographed as it passed through a slit of light. So, photographs show the blade in the plane of the light, and — at least from indications as near as you can tell — this roll-up occurs almost simultaneously or very close to the trailing edge. It would appear (to me, anyway) that, due to the $\left(\frac{1}{\underline{r}_1}\right)^3$ effect, if the vortex is close to the rotor, this might have a large effect on the loading itself.

MR. CRIMI: Yes, I would certainly agree with that.

MR. GRAY: Thank you.

CHAIRMAN DAVENPORT: I think I can add a piece of commentary there, Robin. We have done a test — not on a rotor but on a semispan airfoil model with pressure taps on it — and, if you are going to get right down to brass tacks, you not only have to account for a roll-up, but you also have to account for the fact that the vortex is being trailed from the lateral edge and comes up and over the top of the blade. You can get very weird pressure distributions on that very outboard end.

Do I have any more questions from the floor? (No response.) Well, then, I'm going to ask one more myself. You can't get away, Peter — especially without leaving the little [microphone] clip.

I noticed that you used tipped vortices, and that is a perfectly reasonable thing to do in the need for simplifying a model to get answers. But, if you take a look at it closely, both of those vortices — on one side and on the other — have the same sense of circulation so that, if you took a circuit around the wake far downstream, you would get a net circulation about it. I wonder if you have given any thought to adding, maybe, a very diffuse kind of representation of the inner vortex? We know that a concentrated root vortex isn't really there, but there is a smeared-out one. Do you think that would make any difference?

MR. CRIMI: I don't believe so. We freely admit that we aren't satisfying all of the laws of conservation and circulation. There were some calculations made at the [Cornell Aeronautical] Laboratory using a vortex ring model for the hovering case, and the root vortex that isn't there was represented by a ring [which was] concentric with the outer ring representing the tip vortex. It was found that, invariably, the inner ring convected itself directly up, while the outer ring was convecting itself down. So, it would appear, in that case at least, that the inner vorticity was just removing itself from the flow.

CHAIRMAN DAVENPORT: Well, thank you very much, Peter. I think the next paper is very likely to shed some light on all of these things.

CHAIRMAN DAVENPORT

The next paper is on the Movement, Structure, and Breakdown of Trailing Vortices from a Rotor Blade.

The authors are Ian Simons, Roger Pacifico, and Jeff Jones.

Mr. Simons was graduated from Southampton University in 1963 with a Bachelor of Science in Aeronautical Engineering degree. From 1963 to the present, he has continued at the University on helicopter rotor aerodynamic research under Dr. J.P. Jones. His main interests have been in the induced velocity aspects of rotors. Later this year, he will be joining Bölkow in Munich as a development engineer in their helicopter division.

Mr. Pacifico, after serving three years in the Royal Air Force, joined Canadian Marconi in Montreal where his work involved electronic equipment. He returned to England in 1957 and studied electronic measurement at Southampton College of Technology. He joined the University in 1959 in the electronics section of the Department of Aeronautics and Astronautics. For the past four years, he has been engaged on instrumentation associated with helicopter research programs. This has included design and development of small pressure transducers.

Dr. Jones graduated from the University College Southampton in 1952 with a University of London Bachelor of Science in Engineering. [He was a] research student, working on rotor blade wake flutter and behavior in forward flight from 1952 to 1954; lecturer in Aerodynamics at the University of Southampton from 1954 to 1959 — research interests in rotor aerodynamics and in the flow over highly swept wings; senior lecturer from 1959 to 1961; and Westland reader from 1961 to date. He may explain these academic gradations which are used in the United Kingdom . . . I am afraid I am lost.

In 1955, he received a summer scholarship to M.I.T. where he worked with Rene Miller. In 1964, he was appointed visiting associate professor at M.I.T. Papers on rotor aerodynamics and dynamics, vortex flows and stability, and on anaiog and hybrid computation have been presented by Dr. Jones.

I now turn the floor over to Dr. Jones for his presentation.

THE MOVEMENT, STRUCTURE, AND BREAKDOWN OF TRAILING VORTICES FROM A ROTOR BLADE

by

J. P. JONES, I. A. SIMONS and R. E. PACIFICO

Institute of Sound and Vibration Research,
University of Southampton, England

[Paper contained in Volume I of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN DAVENPORT: Thank you. That was very interesting. I will immediately open [the meeting] to questions.

QUESTION: Ray [Raymond A.] Piziali, Cornell Aeronautical Laboratory, Inc.

What was the disk loading for these pictures where you showed the vortex going up through the disk? And, were the blades able to flap? And, therefore, what was the tip path plane angle relative to the freestream?

MR. JONES: I can't answer the one about the tip disk loading, because we didn't have a balance on this particular setup. But, it is a hinged blade. There is cyclic effectively by tilting the shaft. What was the other [question]?

MR. PIZIALI: I [asked] what was the tip path plane, then; if the blades were free to flap, was the tip path plane actually tilted aft slightly?

MR. JONES: It is tilted aft relative to the shaft, but it is tilted forward relative to the freestream. There is, as it were, thrust on the rotor.

MR. PIZIALI: Thank you.

QUESTION: [William H.] Rae, University of Washington.

Did you ever try putting the smoke in at either 90 or 180 degrees?

MR. JONES: Yes. We haven't tried it in quite the detail that this has been tried. But, if smoke is introduced there, the thing that becomes very obvious is the wake distortion which Dr. Crimi was talking about earlier and which Tararine shows very clearly, where, [for] these low tip speed ratios, the vortices almost come up again to the plane of the disk.

MR. RAE: Yes. We have done some similar work [where] we've had the wake go down and hit the floor, go forward along the floor, and up the walls.

MR. JONES: There certainly is a big wind tunnel wall effect. It is something which has to be taken into account, I think. I don't think there is any problem in this particular set of circumstances of the vortices hitting the floor. I think that their radial position is very much altered by the presence of the walls. If they hit the floor, it is way behind the pylon.

CHAIRMAN DAVENPORT: We have a question from that end of the room.

QUESTION: Frank [A.] DuWaldt, Cornell Aeronautical Laboratory, Inc.

My questions are: Is the vortex forming or is the smoke forming in your estimate of the time for roll-up? And, secondly, do you have any hope of deducing something about the strength from the velocity along the pylon when apparently the vortex is behaving (at least locally) as though it were being translated under the influence of the image?

MR. JONES: The first question was: Is the smoke going up and not the vortex? Is that right?

MR. DuWALDT: In effect, I am asking [if there is] something in the smoke visualization which reflects the inertial characteristics and the exit velocity of the smoke tube? I really don't know; I am asking for an opinion.

MR. JONES: I don't think so. First of all, you do get movements of the smoke under the diffusion, and this seems to move fairly well. If you alter the smoke injection velocity, it doesn't seem to make very much difference to what is happening. This is obviously one of the things which we [have] tried to get in the process of getting the pictures. Apart from that sort of qualitative answer, I can't answer that particular question. But, I would guess it is the roll-up process and not the smoke rolling up.

MR. DuWALDT: My second question was: Can you tell anything from the vertical movement of the filament along the pylon? Can you deduce from this anything about the energy, say, contained in the core?

MR. JONES: Well, we could, but it would be a fairly complicated calculation. But, I think it could be done. I think we get more information about the strength of the vortex by just looking at the hot wire records and taking direct measurements from them. It would be just as reliable, I think, in such a circumstance.

QUESTION: Ian [S.] Gartshore, McGill University.

I gather when you were measuring with your hot wire that you oriented it so that the hot wire just picked up the velocities which were rotating around the core of the vortex.

MR. JONES: Yes.

MR. GARTSHORE: I wondered if, by re-orienting the hot wire, you were able to pick up any other components. There seemed to me to be some evidence in your photographs that there was a longitudinal — when I say longitudinal, [I mean] along the core of the vortex — a longitudinal velocity.

MR. JONES: Along the core?

MR. GARTSHORE: Yes, along the core. There seemed to be . . .

MR. JONES: That means, into the plane of . . . ?

MR. GARTSHORE: That is right, into the plane of the . . . the photograph here . . .

MR. JONES: That wouldn't return upon the wire; that would have been parallel to the wire direction.

MR. GARTSHORE: Yes. That is right. I wonder if you re-oriented your wire so as to be able to pick up any other components of velocity — namely, this one.

MR. JONES: Yes, I quite agree. This is something we want to do next, but we haven't done it yet. I do agree that we would like to see what is happening inside the vortex core.

QUESTION: [D.N.] Meyers, Piasecki Aircraft Corporation.

I don't have a question, but I would like to comment on the question of this gentleman over here [Frank DuWaldt]. It seemed to me that the smoke was emitted continuously, whereas you could see the roll-up happen periodically when the blade passed. That seems to me pretty good proof that it is the vortex rolling up and not the smoke.

MR. JONES: Yes.

QUESTION: Ray Piziali, again. [Cornell Aeronautical Laboratory, Inc.]

With regard to the same thing again. I think, as I understood Frank [DuWaldt], what he was really asking was . . . the smoke which you are observing are not stream lines or path lines, they are really streak lines.

MR. JONES: They are streak lines, yes.

MR. PIZIALI: It is not what you are observing here are there . . . like a tail of . . . like a little galaxie or something. It is not rolling up; it is just the effect — they are streak lines, that's all. You are not seeing the rolling up process. It is just a visualization of the vortex as it exists.

MR. JONES: Yes, but the question is — I thought the question was — does it take the sort of time that we were seeing for the particles of smoke to accelerate to the local flow velocity? I wouldn't have thought so personally; I would have thought they accelerated around more quickly. I think that was your question, wasn't it, Frank?

MR. DuWALDT: Yes.

QUESTION: [Raymond L.] Johnson, Mississippi State.

In regard to that same question, I believe that I saw on some of the movies that when your smoke was introduced — not continuously but discretely — it seemed to enter all of these vortices, even the ones that had been formed in the previous rotations. This would substantiate again that the vortices are here.

MR. JONES: Yes. Thank you very much.

QUESTION: [Robin B.] Gray, Georgia Institute of Technology.

I would like to speak [about] the same question. In the case that I have studied, I introduced the smoke at the blade tip — continuously from the blade tip. It seemed that this vortex was formed immediately at the trailing edge. I believe (and I would take either side . . . I could argue both ways) one, my smoke was trapped within the vortex and could not fully make the vortex core visual. On the other hand, I could look at your data and say [that] what you see rolling up is really the smoke rolling up around the vortex core in the potential part of the flow.

That's a comment. Now, a question comes to mind: Have you superimposed the core diameter that you measured from your hot wire experiment on your flow visualization?

MR. JONES: Yes. But obviously they are not comparable.

MR. GRAY: Yes. This is what I was wondering. In other words, it would appear that a 1/10th chord length would probably be within the clear eye?

MR. JONES: Yes. That is so. But, on your point about the vortex rolling up immediately, we did carry out a series of experiments on a wing which was about the last eighteen inches of the blade — no, the tip. Smoke was introduced, as you say, at the tip, and then we observed the rate of roll-up by introducing light in vertical planes at various chordwise stations downstream.

Because of the geometry of the tunnel, it was not possible to establish where the roll-up was completely finished, but it certainly did not roll up immediately. The roll-up process began immediately, but the growth of the vortex to its final size was, roughly speaking (on that particular model), about the semispan, which would again fit in with what we know from previous experiments and from our experiment.

MR. GRAY: Do I understand that you took a section of the blade and treated it as a three-dimensional wing?

MR. JONES: Yes, that is right.

MR. GRAY: Well, my feeling is that this is a horse of another color. I don't have anything to defend here. It's just that, in my view, it is not clear yet whether it occurs almost immediately [or not] . . . because my vortex core does not grow except that it appears under the viscous action (in other words, the action of breakdown through viscosity).

MR. JONES: Yes, sure.

MR. GRAY: It is very tight, and it also has a very, very minute — on some photographs you can't see it — a clear eye in it.

May I ask another question . . . this concerned me very much. Do you have any suggestions as to what you would call the vortex core? Would you let me give you some examples? Take the line integral around the core and say [that] I have 99 percent of the circulation — is this the core? Since we are dealing with a real fluid, we are not going to have any clear cutoff . . . I'm comparing this to the boundary layer.

Or, taking Lamb's solution for the ring vortex, would you say the core is the diameter that satisfies that equation? I blow a vortex ring, I measure its velocity, its size, [and] its circulation, and then I plug it into this equation and I come out with the vortex core.

MR. JONES: I think I would stick to the definition based on circulation — you know, the integral of the circulation — so that this came to something like 99 percent. I think this is a lot more satisfactory than any definition based upon a theory for something which may not really exist.

CHAIRMAN DAVENPORT: I think we have time for about one more. This one is getting so interesting, it could go on and on.

QUESTION: Jack [Anton J.] Landgrebe, United Aircraft Research Laboratory.

I have a question relating [to] your opinion as far as the relationship of the pictures you see here in the dissipation of the vorticies. Do you feel that observing the smoke dissipate gives you a true picture of the actual vortex dissipation, or do you think it is just the smoke particles kind of washing out of the picture?

MR. JONES: Which one are you talking about . . . just the normal tip trailing vortex — not the wind direction with the pylon?

MR. LANDGREBE: That's right. As you see these vorticies passing down, you might see three or four actual cores, and then they seem to dissipate.

MR. JONES: Well, on that point of dissipation, I didn't make much of it. I think this is a separate question. What may happen is that the vortex comes very close to another blade at some stage in the azimuth, which precipitates this thing called breakdown. This is our view of the sudden disappearance of the eye.

But, on this question of the smoke showing the diffusion or the rate of change of growth — the rate of growth of the vortex, I think Ian Simons has done something on this.

[Directing comment to Mr. Simons]

Do you have any comments you would like to make on that? [You can] get rid of that one.

MR. SIMONS: Well, I think that, when we first did the test, we looked at the size of the hole in the smoke. We were presuming that this was the actual core — actually, we went in the wind tunnel and measured it with a ruler. It was increasing at a fair rate from about 0.5 inch somewhere near the blade to 1.5 or 2 inches when the vortex was 360° [or] 480° of azimuth old. But, when we put the hot wire in (as you saw), the core sizes more or less stayed constant with time — over the range we measured anyway. So, increase in size of the hole in the smoke is probably just due to centrifugal effects on the smoke particles moving outwards.

CHAIRMAN DAVENPORT: I think this might be fun to pursue in the party that is scheduled for later today. And, in order to have time for it, I think I'll end the question period now. Thank you very much, Dr. Jones and associates.

I feel there are some things worth summarizing here.

First, I think the first paper in this afternoon's session indicated that there may be a

revolutionary approach to the rotor. The other event that may indicate something like that is the results shown for the Durand jet flap rotor as it was tested at Ames awhile back.

The other papers seemed to be centered around a major issue right now and that is getting the vortex theory of rotors to work better than it has. Three years ago the question was getting a vortex theory of rotors; now, it is a question of getting it to work well. Both Dr. Crimi and Dr. Jones shed a good deal of light on this. Mr. Paglino's paper rather indicated the need of it. I suspect that his load correlations might have [been] a good deal better if he had a distorted wake in his theory, such as might have been supplied by the results that may follow from these other papers.

I would like to comment that, because of the fiendish complexity of the flow which was also apparent even for the hovering case as it was discussed for props this morning, it may be that marriage of these two techniques — flow visualization and the vortex theory — might lead to some very fruitful results in a fairly short time period.

From the comments we had today, I think it is darn clear you have to be very careful about how you smoke. You can have smoke particles going around in circles, and they are not a vortex — they are fluid particles near one, and they can be in a potential part of a flow (as has been pointed out); so we must be very careful.

I think those are the highlights of what we have heard, plus the osmosis type interaction that I think everybody at a meeting like this gets.

I am now going to turn the meeting over to Dick White [Cornell Aeronautical Laboratory, Inc.]. By the way, Dick, thanks to you and John Yeates [U.S. Army Aviation Materiel Laboratories] for organizing this whole affair. It's been a lot of fun.

TECHNICAL SESSION III
THURSDAY MORNING, 23 JUNE 1966

SESSION CHAIRMAN

J.W. WHITE

U.S. Army Aviation Materiel Laboratories

Two months ago, I wondered what I might say in opening this session and two days ago I wasn't sure I'd be able to say anything, so I will apologize for my southern growl, this morning, and [for this] cold.

In opening this [session] with this apology, I would like to make a comment on this over-discussed, under-practiced subject of communication.

Frequently, we tend to be parochial in our technical specialty or our technical home. We champion the excellence of propulsion or aerodynamic systems or structures, and perhaps we forget that a satisfactory V/STOL aircraft is a delicate combination of sensitive compromises between these systems and subsystems, and, from this point of view, I hope our session will enjoy the same degree of questioning today that we had yesterday to discuss these significant details and permit a better understanding of the trade-offs and compromises within the bounds of the papers that are presented this morning.

The first paper in our session [is] "Predicted and Measured Performance of Two Full-Scale Ducted Propellers," by Dr. Kriebel and Mr. Mendenhall.

Dr. Kriebel has been employed by Vidya Division of Itek since 1960. He has been concerned with the analysis of flow of particle-laden gas through rocket nozzles and shock waves. He has performed theoretical analyses to predict the aerodynamics of parawings, the dynamic stability derivatives of ducted propellers, the interference between a hull and a ducted propeller, and the interference between a pair of ducted propellers.

From 1956 to 1960, he was with the Stanford Research Institute. From 1953 to 1956, he was with M.I.T. [as] a research assistant in the gas turbine laboratory. From 1951 to 1953, he was a Lieutenant in the Army Ordnance Corps. And, from 1950 to 1951, [he was] at Hiller Helicopters.

He graduated from Massachusetts Institute of Technology in 1950 with a Bachelor of Science [degree] in Mechanical Engineering. In 1956, he received Master and Doctor of Science [degrees] in Mechanical Engineering. He has been the author of numerous publications.

Mr. Mendenhall joined Vidya in 1964 and is now assigned to the Applied Mechanics Department. He has worked in the areas of fluid mechanics and orbital mechanics.

In 1963, he was at the Ames Research Center, and, while attending the University of Wichita from 1960 to 1962, he was a student engineer with the Boeing Company.

He received a Bachelor of Science degree in Aeronautical Engineering from the University of Wichita in 1962 and a Master [of Science degree] from Stanford University in 1964.

The paper will be delivered by Dr. Kriebel.

PREDICTED AND MEASURED PERFORMANCE OF TWO FULL-SCALE DUCTED PROPELLERS

by

A. R. KRIEBEL and M. R. MENDENHALL
Vidya Division, Itek Corporation

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN WHITE: Now, for the question period, we will operate just as we did yesterday. For the benefit of those who are transcribing these questions and comments into written material for us later, please clearly state your name and affiliation.

QUESTION: Gary [R.] Ludwig, Cornell Aeronautical Laboratory, Inc.

As I understand it, the separation points on the first series of pressure distribution slides were predicted — is that correct?

MR. KRIEBEL: Yes.

MR. LUDWIG: Do you have any indication of how good this is? In some cases, the pressure distributions showed a quite large pressure rise after the predicted separation point but still before the propeller; I find this a little odd.

I would think that maybe the predicted separation point was too far forward. Is there any data to indicate how good these predicted separation points are?

MR. KRIEBEL: Generally, the separation points predicted from the predicted pressure distributions lie aft of the ones using the measured pressure distributions, because the pressure gradient is less adverse up to the propeller for the predicted curves.

MR. LUDWIG: I meant the measured separation.

MR. KRIEBEL: However, for the predicted curves, we would find separation immediately at the propeller where the discontinuity in pressure occurs.

There really isn't too much difference between the measured and the predicted separation points, in axial flow at least, since the predicted ones are at the propeller, and the predicted ones using the measured curves are only slightly up forward of the propeller.

MR. LUDWIG: These are both predicted separation points, though.

MR. KRIEBEL: Yes, that is right.

MR. LUDWIG: Now, I meant, is there any indication [as to] how good either of these are with respect to an actual separation point?

MR. KRIEBEL: We have no information as to the actual separation point whatsoever.

MR. LUDWIG: I see. Thank you.

QUESTION: Kaz Korzak, Piasecki Aircraft.

I am wondering why your investigations were limited to angles of attack of 80 degrees? It seems important also to extend this range to the full range of 180 degrees?

MR. KRIEBEL: We cover the angle of attack range which was covered in the wind tunnel data, which went up to 90 degrees — but not beyond 90 degrees. I am not sure why neither of us went beyond 90 degrees.

MR. KORZAK: I just would like to explain why I would be interested in that: Because, in hovering with wind, wind can come from any direction?

MR. KRIEBEL: Quite true. We haven't considered that problem at all.

QUESTION: Frank [Franklyn J.] Davenport, Boeing-Vertol.

I would like to comment on the comment. This is sort of rotationally symmetric, isn't it? So, if we cover from 0 to 90, haven't we got from 90 to 180 automatically?

MR. KRIEBEL: No.

QUESTION: Irv [I.H.] Culver, Lockheed Aircraft Corporation.

I think several people made the comment on the comment which was: No, you haven't. But, the question I have is: Have you tried tuft techniques inside the duct to determine the actual separation conditions at all, or is it possible to use tufts? You know what I mean — little pieces of thread taped on to find out where it stalls.

MR. KRIEBEL: Yes. All of the experimental work was done at the Ames Research Center. In the case of the smaller duct, they did have tufts on the inner duct lift. This is one indication of duct stall; however, there wasn't detail reporting of the location of separation.

QUESTION: Don [D.E.] Ordway, Therm Advanced Research, Inc.

First, I want to compliment you, Bob. Because, when they asked for 180 degrees . . . I think just going through the range you did is a real valiant attempt. But, what I wanted to ask about was the model which you said you were planning with the finite blade number — was that correct — or was it still going to be an acuator disk with a variable circulation distribution?

MR. KRIEBEL: We have used a finite blade number representing the blade element theory. However, we represent the trailing vorticity as a series of concentric vortex cylinders. We do not have the spiraled vortex.

MR. ORDWAY: So, you wouldn't have a periodic variation on the blade loading itself?

MR. KRIEBEL: No, that is right.

MR. ORDWAY: Well, one important point I would like to make is that, from our analysis, we have found that this periodic loading that you will have on the blade at angle of attack gives rise to a steady side force and yawing moment which we feel is significant as far as stability is concerned — I mean the application to the complete aircraft.

MR. KRIEBEL: Yes, we have also estimated recently the side force in yawing moment due to the finite blade number; however, we found this force in moment seems to be relatively small.

QUESTION: [F.N.] Piasecki, Piasecki Aircraft Corporation.

I would like to comment that this tuft investigation of the boundary-layer separation is not a steady state. We have tufted several ducted propellers, and it seems that it is a function versus time. There seems to be a gulping, so that the separation of the boundary layer is not something that is constant with velocity or angle of attack or chord position. It is also a function of a growth — extremely hard to explain.

Incidentally, we are interested in the 180 degree flow, because we have flown two machines where this is important: One, the aerial jeep, where you can come down so your flow is this way, [speaker motions with hands] and, in the ring tail compound work, where you can fly backwards.

MR. KRIEBEL: I certainly agree with your comment that further information about the boundary layer on a duct is highly desirable experimental information — on any duct.

MR. KORZAK: I have another question. Do the pressure distributions which were shown by you — they all refer to the average, I guess — when the propeller passes over a certain cross section . . . there is a definite change which is very important from the structural point of view of the duct. Was there any work done in this respect?

MR. KRIEBEL: These unsteady effects can be quite large, I understand, near the propeller. We have not considered them, although they have been considered in some detail at Therm [Advanced Research, Inc.].

QUESTION: Wayne Wiesner, The Boeing Company.

Tony, have you made any calculations of flat pay drag at zero power or zero thrust? And, also, what is the drag increase due to separation?

MR. KRIEBEL: We've looked at the drag on the duct — we've estimated it — and it seems to be always small compared with the total thrust; however, it can be larger than the duct thrust at high advance ratios and light loading. I really don't know what the effect of separation on the drag is. I would think it would decrease it.

QUESTION: [Frank A.] DuWaldt, Cornell Aeronautical Laboratory, Inc.

With the discontinuity indicated in the pressure distribution, I find a little difficulty in visualizing the separation point aft of the jump.

MR. KRIEBEL: Well, I think the effect of this discontinuity in pressure is similar to what you would have with a normal shock impinging on a wall, where you have a shock-wave boundary layer interaction. There is a sudden rise in pressure in both cases, although the static pressure on the wall rises gradually because of the shock wave boundary layer interaction.

CHAIRMAN WHITE: Time for one more question.

QUESTION: [Franklyn J. Davenport, Boeing-Vertol]

Well, I'll try again. I think in terms of that pressure rise across the disk that this is quite different from a shock in one sense in that you do have some clearance between the tip of the propeller and the shroud itself and that it is going to involve a series of vortices coming right off in the boundary layer inside there that should promote mixing. Despite the very large pressure rise, I should think that you have a very effective kind of vortex generator that should tend to prevent separation and make it a much better behaved flow behind the prop than you would expect behind, say, an actuator disk, if it were really an ideal one.

MR. KRIEBEL: This is certainly true. We would never expect to see the discontinuity in a measured pressure distribution, not only because of the analogy to a shock wave boundary layer interaction — which would acquire only if a true actuator disk [was there] — but also, as you say, there really isn't an actuator disk there. We're representing all of the blading — in one case, three blade rows — with an actuator disk. There is certainly a finite width of blading there, and also a finite spacing of blades. There would never actually be a discontinuity there, although the measured pressures seem to fair through this discontinuity reasonably well.

MR. DuWALDT: I still persist [that] the theoretical pressure distribution didn't know anything about the smoothing effects of either a mixing or another interaction effect, and there was a jump — an infinite slope — at that point; that is what makes it so difficult for me to visualize a computed separation point aft of the jump.

CHAIRMAN WHITE: Thank you.

CHAIRMAN WHITE

The next paper this morning is "Aerothermodynamic Performance of a High Bypass Tip Turbine Cruise Fan System."

This paper is being presented by Mr. Gene Smith. He is Senior Engineer of the Test and Evaluation at the Lift Fan Systems Operation of the Advanced Technology and Demonstrator Programs Department of the General Electric Company at Cincinnati, Ohio.

Gene graduated from the University of Cincinnati in 1953. From 1953 until 1955, he was in the United States Army as a Missile Test Officer. From 1955 until the present, he has been with the General Electric Company. He has spent the last eight years with the Lift Fan Systems, in aerodynamic performance and test evaluation, diverter valve design and test, and XV-5A aerothermodynamic performance. Now, I turn the meeting over to Mr. Smith.

AEROTHERMODYNAMIC PERFORMANCE OF A HIGH BYPASS TIP TURBINE CRUISE FAN SYSTEM

by

E.G. SMITH

General Electric Company

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN WHITE: We are open for questions now. Please, again, identify yourself, because names are very difficult to understand in transcribing.

I have one question, Gene. You might mention the pressure ratio of the fan?

MR. SMITH: I tried to avoid it.

CHAIRMAN WHITE: That may be one of the problems with the drag.

MR. SMITH: I tried to avoid that question. The fan designs we are now looking at are 1.3 pressure ratio plus (up as high as we probably can get up on a single stage), and this little X-376 is pushed to get 1.1. There were a lot of compromises in the nacelle design. I tried to avoid [this area].

If you looked at the first picture of the full-scale cruise fan geometry, it was a nice clean installation. Then, when I showed you the model, the after body, the plugs sort of changed shape. These changes in shape were partly due to the compromises of the lower pressure ratio. Is this what you were asking in particular?

CHAIRMAN WHITE: Yes.

QUESTION: [William C.] Schoolfield, LTV, Dallas.

Is this system supposed to provide vertical take-off?

MR. SMITH: Yes, it can. We do get quite a good augmentation ratio out of it. I don't want to quote a number — I really don't know — as compared to the core engine. But, in the remote case, for example, where we install a core engine inside the fuselage of an airplane, you can rotate the system once you get a rotating seal across the duct, and then it can be used for a direct lift system. If you don't worry about engine out and cross coupling of the two fans to one another, you can go to a close coupled arrangement; but, there you are running the risk of a single engine driving a single fan, and this has been avoided at least whenever we use our fan system for a direct lift.

QUESTION: [V. J.] Di Sabato, Pratt and Whitney Aircraft.

If you were to use this system on a VTOL aircraft, would you use the plug in the tail surface?

MR. SMITH: Presently, we still have plugs in them, yes.

MR. DI SABATO: From the looks of the configuration, if you were to rotate it, you may have a problem in that the airplane may hobble down the ground because the plug may hit the ground surface.

MR. SMITH: No, it is still clearing the ground. There are possibilities of other nozzle designs, but the one we are building right now — a development piece of hardware — is running with a plug nozzle on it. We want to find out if that inflatable boot also works.

QUESTION: [A. Z.] Lemnios, Kaman Aircraft.

Have you considered a fan configuration with variable pitch blades at all?

MR. SMITH: I want to apologize for one thing; I am a test man, first of all. It has to be real hardware before

I get ahold of it. I don't want to quote what our design people are doing right now. But, there are variable geometry fans in study cycles right now. [There is] nothing in the hardware phase, though.

QUESTION: [E. P.] Cockshutt, National Research Council.

I would like to inquire more about the variable area nozzle required with the fan. Can you give us an indication of the area variation of the range?

MR. SMITH: On this 1.1 pressure ratio fan, I didn't put that on the slides, [but] it is in the report. There is one figure that shows the area variation. I think it went down as low as 50 percent at high speed points, so we have to have a 2:1 area variation. The 1.3 pressure ratio is the number I think that is down around the order of 30 percent — 20 to 30 percent — depending on how much you want to compromise cruise versus your static operation.

MR. COCKSHUTT: Do you feel two-position will be enough?

MR. SMITH: Yes. We have studied that, and the penalties in mid-speed range are not very great.

MR. COCKSHUTT: I have a second question. Do you use complete continuous admission to your turbine in this arrangement, or is this a partial admission turbine?

MR. SMITH: Both. Either or both. The reason I said both [is], if you run a single engine cross-coupling arrangement, you probably run 50-50 for engine out capability. So, it would be either half or whole, if you had engine out. We are not building the 80-inch fan as partial admission, but the XV-5 is partial. So, we have both that way too.

MR. COCKSHUTT: But, from nacelle size, I presume you would eventually have to have . . .

MR. SMITH: [Interrupting:] That's what I mean. We haven't iterated to that extent yet.

QUESTION: [C.H.] Carper, AVLABS.

In your tunnel data, did you get any effect at a high angle of attack on the fan flow and pressure distribution as it might be affected by the engine itself?

MR. SMITH: Are you asking [if] the engine inlet going in the front of the fan gives us a problem?

MR. CARPER: Yes, at a high angle of attack.

MR. SMITH: We didn't see the engine in the fan flow, at least with the instrumentation we had.

MR. CARPER: What angles of attack did you go to.

MR. SMITH: I don't remember the exact numbers. The model was so large and the forces were so high that we were limited on angle of attack excursion. If I remember right, I think we got 8 degrees excursion at 0.5 mach number, 6 degrees at 0.7 mach number, and about 5 degrees at 0.8 mach number — or something like that. They weren't big excursions because the balance limits were holding us back.

To make another comment, a similar model has been run at NASA Ames; maybe you have seen some of the results. There, they went through complete angle excursions at low speeds. And, again, of course, we didn't have any problems with the engine inlet getting in front of the fan in that case.

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

I have to comment on the fact that NASA Ames had the engine on the top of the fan and not on the bottom, so angle of attack excursions wouldn't have placed it in front of the inlet anyway.

MR. SMITH: I'm sorry. I thought of that after I said it, but I didn't want to correct myself. That is right. Negative angle of attack in the Langley wind tunnel was nose up. We changed it around. The model was mounted inverted in the tunnel . . . I am trying to think of the reason exactly — there was a reason given. I wasn't [involved] in the original design of the installation, but there was a reason why the thing is mounted inverted relative to normal installation. I stand corrected on that.

QUESTION: [F.N.] Piasecki, Piasecki Aircraft Corporation.

The earlier question asked about how this unit would be used in a VTOL system . . . I didn't quite get your answer. Could you please elaborate?

MR. SMITH: Well, one way it could be used is in a remote installation where the core engine is removed from the fan system — it's not an integral part of the nacelle. And then, by making a rotating hot gas duct seal, you can rotate the complete cruise fan — including the nacelle — as part of the aircraft propulsion system. You can put it at 90 degrees, or reverse thrust even, and rotate it into the aligned position for cruise. Some of the aircrafts are being proposed this way. Again I have to apologize. I am not in the design part of that . . . I just see the pictures the other people turn out.

CHAIRMAN WHITE

The third paper [of this session is entitled] "Thrust Deflection Nozzles for VTOL Aircraft." It will be given by Mr. Vincent J. DiSabato, Pratt and Whitney Aircraft, East Hartford, Connecticut.

Mr. DiSabato is a Senior Engineer in the inlet and nozzle group. He directs the aerodynamic analysis and testing of nozzles and thrust deflection systems. Consequently, he is responsible for the wind tunnel scale model testing and the supervision of the design of nozzles and deflection devices.

His experience from 1961 to present has been in the inlet and nozzle group. From 1960 to 1961, he was involved in test operations and was in charge of liquid metal turbine and turbo-pump test stands. From 1958 to 1960, he was an Instructor at Tri-State College, Aeronautical Engineering Department.

He graduated from Tri-State College with a Bachelor of Science degree in 1958.

THRUST DEFLECTION NOZZLES FOR VTOL AIRCRAFT

by

V. J. DI SABATO

Pratt and Whitney Aircraft Division of

United Aircraft Corporation

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN WHITE: Do we have any questions?

QUESTION: Stanley Youth, Grumman Aircraft.

I would like to register some serious objections concerning the ground rules with which we are comparing the lift engine and the deflected engine.

First of all, your engine has obviously been sized for VTOL operation. To use a ground rule where we use none of the engine weight in the thrust to weight ratio, as compared to the thrust to weight ratio of the lift engine where the basic engine is sized to the cruise engine, is really not a fair comparison.

MR. DI SABATO: The lift cruise engines, when you use lift engines with them, can be sized for a horizontal condition and not for take-off. A lot of times, the horizontal condition is a lot less stringent than take-off condition. So that you are effectively making the cruise engine more efficient when you size it in this way. In a VTOL aircraft not having lift engines, you would have to size for take-off and then, of course, the thrust of wake would be pretty bad.

QUESTION: [E. P.] Cockshutt, National Research Council.

I would like to inquire, with regard to the ventral nozzle system, whether constant

nozzle area is achieved at the intermediate conditions between horizontal thrust and vertically downward thrust? I got the impression that the nozzle area which the engine sees was in fact changing, and this, of course, would be a serious penalty.

MR. DI SABATO: [That is] right. You would have to change the area as you went from a horizontal or from a low amount of flow in the nozzle to larger amounts of flow so that you can get the correct area when you get to a vertical point.

MR. COCKSHUTT: So you either need additional variable geometry or you have to make it a two-position system?

MR. DI SABATO: Well, if you use the clam shell blocker or some sort of a system where you're closing the axial flow off and then uncovering the ventral system, you are in effect automatically varying the area of the ventral nozzle.

MR. COCKSHUTT: Yes, but I question whether the gas generator sees a constant nozzle area?

MR. DI SABATO: This may be true. There will probably have to be some area variations sometime in the ventral system itself. But, in the tests we performed here, we didn't allow for it.

MR. COCKSHUTT: This was not a criterion of your test?

MR. DI SABATO: No.

QUESTION: Abe [A. C.] Adler, Hughes Tool Company.

Just a comment on the lift engine: you should be comparing them with the installed thrust to weight ratio and not the bare thrust to weight ratio, so that you would be talking about numbers of say 12 to 15 rather than your 15 to 20.

MR. DI SABATO: Right. I gave them the benefit of the doubt.

CHAIRMAN WHITE: Are there other questions?

(No response.)

I had one question. Are the thrust to weight [ratios] that are listed on the sheet for augmented temperatures or dry temperatures?

MR. DI SABATO: For the swivel and two ventral systems, there was no augmentation in the vertical mode. For the aft-hood defector, there was augmentation.

QUESTION: [Robert R.] Piper, AVLABS.

Looking at your figure of ground proximity effects, I was wondering if I am missing the boat. Is this a serious design problem? Can you conceive of an airplane with a landing gear so stubby that the exhaust would in fact be only 75 percent of the exhaust diameter above the ground, or am I misunderstanding the curve?

MR. DI SABATO: Yes. The only reason I put that in there was to show that, from an engine standpoint — which the curves were tested for — with no external surfaces on the nozzle that we tested (this would be as close as you could get or put the engine and still not affect the engine) it would be approximately 5 to 6 percent of the duct diameter. But, I also said that, when you want to install this in an airplane, you are, of course, going to have the airplane surfaces to contend with. The location of these deflection nozzles in the aircraft become important, because you can get this pressurization on the bottom surfaces of the airplane, or you could actually get suck down, depending on the installation. This would be the absolute minimum distance that you could place the engines and still not affect the engine performance.

QUESTION: [John A.] Conway, DeHavilland Aircraft of Canada.

I would just like to make a comment on the last question — on the last statement there. I

might mention the German VJ-101 where the nozzles do come very close to the ground. Of course, due to the location of the engine, they do get, I believe, a positive effect.

MR. DI SABATO: I might say that, on that airplane, they have to, I believe, use a jump take-off in order to get off the ground. When they initially started testing this system, the airplane just sat there; when they burned off enough fuel, then they could take off. They had a suck down.

QUESTION: John [G.] McReynolds, Lockheed Georgia Company.

I think the problem of the ground proximity could be affected by a clustered nozzle arrangement. Do you have any comments on that — where a number of nozzles might be in close proximity to one another?

MR. DI SABATO: We haven't studied any clustered arrangements or any type of aircraft arrangements, mainly because we haven't been really involved with the aircraft people at this point as far as a given airplane configuration that would be used.

CHAIRMAN WHITE

The last paper for this morning's session is entitled "Shrouded Propeller Research at Mississippi State University Leading to Application on the United States Army XV-11A."

The authors . . . Mr. Thompson is an Assistant Professor of Aerospace Engineering at Mississippi State University, where he has been since 1964. He received a Bachelor of Science degree from Mississippi State University in 1961, and a Master of Science degree from Mississippi State University in 1963.

He was a Physicist at the Nucleonics Division, United States Naval Research Laboratory, Washington, D.C., in the summer of 1961. He was a Graduate Research Assistant in the Aerophysics Department, Mississippi State University in 1962 and 1963. He was an Aerospace Engineer at the Marshall Space Flight Center, NASA, Huntsville, Alabama, in 1964.

Approximately half of his time is spent in teaching in the Aerospace Department and half is spent in research in the Aerophysics Department.

The co-author, Dr. Roberts, has a Bachelor of Science degree from London University, a diploma from the College of Aeronautics, and a Doctor of Philosophy degree from London University. He has been an Aerodynamist, a Flight Test Engineer, a Fulbright Research Fellow, and is currently head of the Aerophysics Department, Mississippi State University, where he's been since 1964.

Mr. Thompson will present the paper, and we will hear from Dr. Roberts tomorrow.

SHROUDED PROPELLER RESEARCH AT MISSISSIPPI STATE UNIVERSITY LEADING TO APPLICATION ON THE UNITED STATES ARMY XV-11A

by

J. F. THOMPSON, JR. and S. C. ROBERTS
Mississippi State University

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN WHITE: Are there any questions?

QUESTION: [H. C.] Curtiss, Princeton University.

Certainly, moving backward is a very unsteady situation. I wonder if you can give an idea what the thrust fluctuations were during a run and what the fluctuation in the flow pattern was. It's clear from energy considerations that vortex pattern is shifting around all of the time and can't stay like it was drawn.

MR. THOMPSON: I agree. It is a somewhat unstable situation. I think this is borne out in the fact that we did have these two bistable pressure distributions, type A and type B, at some of the rearward motion speeds. I don't have any instantaneous measurements; we didn't make any instantaneous measurements — they are all manometer measurements [which have been] simply photographed.

We did make several at each location and, generally, the points lay very close together on the pressure distributions. In fact, they were surprisingly close together, except for this bistable effect between type A and type B — in that case, it completely changed. But, we actually found, in some cases, that maybe half of the points would be type A and half would be type B.

All of the type A's would agree in a very narrow band, and then all of the type B's would also agree in a very narrow band.

MR. CURTISS: You didn't have any time histories?

MR. THOMPSON: I didn't have any time histories. They were all manometer measurements taken with photographs.

QUESTION: [Speaker did not identify himself.]

Joe, I would like to compliment you on excellent testing. We have found the stagnation point exactly the way you described it on six different full-scale ducted props — both by smoke studies and by tuft studies and by just handkerchief studies . . . you just take in in your hand and you can feel the stagnation point.

Incidentally, in rearward flight, which is the design condition on our compound helicopter ring-tail, we have gone to 35 miles per hour rearward flight and have had no difficulty in control.

MR. THOMPSON: I thank you very much. I am glad to hear your confirmation. That is the only other study that I heard from.

QUESTION: Kaz Korzak, Piasecki Aircraft Corporation.

I am also very glad to see those results of rearward flights. However, [an] angle of attack of 135 degrees might bring even more unexpected results.

MR. THOMPSON: That is quite possible. In fact, a possible extension we have in mind is mounting this thing on a truck bed and operating at angles of attack between 90 and 180 degrees; although, we've not done anything in that direction yet.

QUESTION: [Ian S.] Gartshore, McGill University.

I was wondering if this situation that you described as bistable is in fact just a separation of the boundary layer on the inside of the shroud

from the nominal leading edge back. It seems it could separate from the leading edge back to the fan and then be re-attached by the action of the fan itself. Did you have any tufts on the inside of the shroud?

MR. THOMPSON: No, I didn't. We didn't have any indication on the inside at all except the pressure distributions. I couldn't rule out your case. That certainly could be possible, although I have no way of knowing on the inside of the shroud.

QUESTION: [A.A.]Perlmutter, Dynasciences Corporation.

Did you have any test data on vanes at the exit of the shroud — deflecting vanes?

MR. THOMPSON: Oh, on the XV-11A?

MR. PERLMUTTER: Yes.

MR. THOMPSON: We do have some test data on that, I think. [Directing comment to Dr. Séan C. Roberts:] Séan, do you want to comment on that or wait awhile? We are in the process of obtaining this data at this time.

MR. ROBERTS: This data will not be available until about another two months. We do have pressure taps on the control surfaces and that data will be available in about two months.

MR. PERLMUTTER: You have as yet nothing to indicate whether or not you have a decreased loss in thrust in that rearward motion?

MR. ROBERTS: No.

QUESTION: [Leonard] Meyerhoff, Eastern Research Group.

One possible explanation for this phenomenon could be that, when you run your shroud rearward,

the profile presented to the flow appears to be a diffusing type inlet, as compared (as I understand it) to an accelerating inlet in the [forward] condition.

Now, if you have a diffusing inlet in the rearward condition, then the flow would tend to separate at the leading edge, and the possibility is that a large separation bubble forms. The outer confines of the separation bubble could be conceived as the new upper surface of the airflow, in which case you would have a fantastic deceleration [which] would account for the extreme curvature around the shroud.

We have run ducts forward and backward on a computer, and we have indeed found that there is an extreme deceleration on these type shrouds that have an accelerating inlet.

In other words, in reverse, they behave like diffusers rather than accelerating inlets.

MR. THOMPSON: Right. One thing of interest [is that] in these tufts above the shroud — and I think you would have to see the research report to see it, because all of the tuft pictures are presented there — the tufts are very regular above the shroud, even when they are pointing into the direction of the relative wind. They are very regular, and, then, maybe in the space of one tuft, there is a shift to the case of being in the direction of the relative wind.

You can almost draw the boundary line between these things. So if it is a separation, it is a very regular laminar bubble in there with very regular flow rather than erratic [flow].

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

I wonder, perhaps, if the analogy here was clear? My only experience with testing a duct in reverse was in a wind tunnel at about 30 knots when the pitch mechanism failed and I suddenly had a reverse condition. This was at about 85 pounds per square foot disk loading and, as you indicate, the entrainment was just as you have shown — there was no basic change in the tufts; they continued as if it were in the forward flight

condition. Under those circumstances, we didn't look for the outside location of the stagnation.

But, the thing I wanted to point out was, this condition is somewhat analagous to the vortex ring state in the free propeller with the boundary interjected. In these conditions, you do have this type of flow. I have done a considerable amount of work in the descending free propeller and have shown situations as you have indicated; that is, a loss in thrust as you begin to go into the negative advance ratio range and then, of course, as you pass on — which you probably have not done — into what corresponds to the windmill break stage, a recovery in thrust and a considerable increase as you go along through the stall state.

I wonder if maybe some of these things are not analogous to this and that these vortices which are being formed are those which would be the tip vortices on a propeller, but the barrier, of course, is interjected. I think the patterns are basically as you have shown them. The entrainment by the high flows through the duct, of course, does entrain the air on the outside. In a 90 degree case, you can show that there is entrainment of the flow as much as 25 to 30 percent of the radius outboard of the outer duct surface.

The only other comment I might make is that, in the descending free propeller, we did make dynamic measurements of the thrust variations. We discovered that there were fluctuations of as much as 60 to 70 percent of the steady state thrust but that they occurred in about 0.2 second period, so that the average level, I would say, would not really be felt by the structure as a whole.

MR. THOMPSON: [That is] very interesting. We, of course, would not at all have picked up fluctuations like that. I would be interested in seeing your results on the propeller also.

CHAIRMAN WHITE: Are there other questions?

(No response.)

I would like to personally thank the authors for their time in preparing these papers at night, Sundays, and at other times, and to thank Dick White for the preparation that has gone into this meeting.

TECHNICAL SESSION IV
THURSDAY AFTERNOON, 23 JUNE 1966

SESSION CHAIRMAN
I. H. CULVER
Lockheed-California Company

I think all of the papers this afternoon fall into a generalized category of confused aerodynamics. If you look at the last paper, the incoming flow is confused with the outgoing flow, if you want to look at it that way. If you run the C_L's high enough, that will certainly happen.

The first paper this afternoon is entitled "The Lift, Drag and Stability of Wings Immersed in Propeller Slipstream." This is by K. P. Huang and N. J. Miller, and, unfortunately, Mr. Miller is not with us this afternoon. Mr. Huang will deliver the paper.

He went to the University of Shanghai and received a Bachelor of Science degree in Mechanical Engineering in 1941, and went to the University of Pennsylvania for a Master of Science degree in Mechanical Engineering in 1964.

Mr. Huang is currently a project engineer in various studies at the Dynasciences Corporation. His work includes analysis and investigation of the effects of propeller slipstream on the performance of wings, and potential of jet ejectors in thrust augmentation for aeronautical applications.

Prior to his present work, Mr. Huang "cut his eye teeth" in Hong Kong on steam boilers and electrical generators, and all that sort of jazz. So, he is really qualified to cut into these heavy subjects. With that, I will turn the meeting over to Mr. Huang.

THE LIFT, DRAG AND STABILITY OF WINGS IMMERSED IN PROPELLER SLIPSTREAM

by

K. P. HUANG and N. J. MILLER

Lockheed-California Company

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN CULVER: As we have been doing, I hope that everyone will state their name and affiliation when asking a question during these discussions.

QUESTION: [Gary R.] Ludwig, Cornell Aeronautical Laboratory, Inc.

I have a few comments, and I would like to congratulate you on your excellent agreement between theory and experiment.

First, I have a question. In the lift force case, where you presented the comparison between theory and experiment, you noted that you had no way of determining the difference between the propeller thrust and the lift experimentally.

Now, in doing the theory, you must have had to separate them and then add them together. Could you perhaps give an idea of how much of this lift increment theoretically was due to the propeller and how much to the wing?

MR. HUANG: I cannot offhand tell how many percent, because [it] all varies [with] the thrust coefficient and the angle of attack.

MR. LUDWIG: Say at high angle of attack, would it be 50 percent propeller and 50 percent wing? Just very crudely?

MR. HUANG: At high angle of attack? Yes, probably. I can't tell the exact [figures].

MR. LUDWIG: The other [question] was with regard to the last statement. There is available now experimental data on lift distributions in axially symmetric flows with shear. At Cornell [Aeronautical Laboratory], we have performed some experiments in this regard, and we have lift distributions. I believe you have Brenckmann's work in your references; there are some lift distributions in there also on the wing.

MR. HUANG: Yes.

MR. LUDWIG: And one thing that is noted in there is that, external to the slipstream, the wing can take on characteristics other than the section coefficients found for a uniform flow. This has been attributed to a bleed-off effect — the boundary layer becomes three-dimensional because of entrainment. So, I wondered about the prediction of stall external to the slipstream by using two-dimensional coefficients which no longer correspond to the actual case. Do you care to comment on that?

MR. HUANG: As I said, this is a very simple theory. We tried to make use of a simple theory to find how the wing initial stall can be found. We haven't investigated what you have just said.

QUESTION: Dick [Richard P.] White, Cornell Aeronautical Laboratory, Inc.

Mr. Huang, I just happened to walk in on (I think it was) your second slide where you presented an expression for ΔC_L, where you expand it in terms of power coefficient, thrust coefficient, $C_{T,S}$. I went and got a Volume [of the Proceedings] to check myself. In that expression, you have terms of $\frac{1}{(1-C_{T,S})}$ and you present results for $C_{T,S}$ equal to 1, and you have finite ΔC_L. I wondered if I missed something, or if the equation presented in [Volume II of] the Proceedings was wrong?

MR. HUANG: Let me explain like this. I just said that the total wing lift coefficient is composed of two contributions — one is basically lift, and the other is incremental. I want to add these together so that the dynamic pressure has to be the same; and, in these equations, the freestream dynamic pressure is used.

Of course, at C_{T_S} equal to 1 — that is, static condition — there is no freestream. So, the freestream dynamic pressure actually doesn't exist. In that case, to determine the lift coefficient, we have to base [our calculations] on the slipstream dynamic pressure. If you want to use that kind of dynamic pressure, we have to multiply everything by $1 - C_{T_S}$. Then the denominator $1 - C_{T_S}$, is eliminated. I hope this answers your question.

MR. WHITE: Thank you.

QUESTION: [R.]Hafner, Westland Aircraft Ltd.

I must apologize for my voice, which is almost non-existent; I hope I can make myself understood. The question I have is mainly this: Avoiding the mathematics of your paper for the moment, can one say, in the case of a fully emersed wing within the slipstream and on the assumption of the safely uniform slipstream velocity, that the wing behaves in that slipstream as in any ordinary airflow — in other words, as in the freestream airflow — the difference being mainly that the slipstream, of course, has a higher dynamic head and the allowance has to be made for the change in direction of the velocity vector? Is this the essence, the mechanism that you are talking about, or are you including here other important factors?

MR. HUANG: Well, essentially, just as I pointed out, this is a simple approach. So, we just consider [that] the slipstream resultant velocity is the vector sum of the induced velocity and the freestream.

MR. HAFNER: If the wing is coming in pretty close to the edge of the slipstream, you have a pretty complex case. But, I would have thought that if the wing is way immersed within the slipstream, can one make this simple assumption without a great risk?

MR. HUANG: I agree with you. This is not as simple; [that is] right.

QUESTION: [A.A.] Perlmutter, Dynasciences Corporation.

Let me answer this question. I was somewhat involved in this, and I think I can give you an answer. If you just take the difference in the velocities into consideration, you will not get the entire answer. This method was not just a superposition of velocities, but it actually calculated the wing lift based on satisfying the boundary conditions of no flow through the wing using potential theory [and] considering the situation first of a wing fully immersed in the slipstream (and, hence, no effect of any outside velocities on any wings not immersed). It turned out that this resulted in a substantial increase in lift as you would have obtained if you would have just used a velocity increase by itself.

MR. HAFNER: Thank you very much.

QUESTION: [H. C.] Curtiss, Princeton University.

I wonder if you could explain why Equation (1) in the paper doesn't reduce to Jones' result when the flow is uniform throughout the field. It seems to be about 50 percent different than the standard slender body result.

MR. HUANG: It should be the same answer. I don't know. We didn't investigate that [difference].

QUESTION: [W. Z.] Stepniewski, Boeing-Vertol.

I don't know if you are familiar with some results — rather recent results — of the test

of the new arrangement of tilt wing force, where the thrust line is much lower from the chord line, and that led to a rather dramatic improvement in stalling capabilities, and so on. Had we investigated that problem of lowering the thrust line with reference to the chord line?

MR. HUANG: In our investigation, the thrust line is assumed to be in the same vertical plane as the chord line. So, the question about how the lift would be increased because of a lowering of the thrust . . . we haven't come into that.

QUESTION: [William F.] Putman, Princeton University.

First, I would like to make a comment with regard to the first comment. The nondimensionalization of the experimental and theoretical comparison implies that the thrust is known — $C_{T,S}$ is thrust over slipstream times disk carrier — and so, then, we can compare just the wing contribution to the forces by subtracting out $C_{T,S}$; is that correct, sir? Are you with me?

MR. HUANG: If I understand you correctly: the relation shows the sum of the contributions due to thrust and the effect of slipstream.

MR. PUTMAN: Yes.

MR. HUANG: I didn't separate these two. There is no available data for the contribution due to the thrust. If you just want to separate this thing from the test data, the only possible way is to just calculate the sign of the angle of attack from the given stress coefficient.

The second point is, we have studied the test data. Actually, even with — what I should say is — with no thrust . . . the $C_{T,S}$ [is] equal to zero. The data are sometimes inconsistent; we really have no explanation.

We tried to subtract also the contribution due to the thrust, but finally we decided that we should put it in, because we don't know how to accurately separate these two.

MR. PUTMAN: My only point was that the form of non-dimensionalization implies a knowledge of the thrust. [In] the specific tests, I do not know if the thrust was measured continuously throughout the angle of attack range; however, through the small angle of attack range that you are concerned with, the thrust does not seem to be that strong a function based on conventional mass of form in testing in many cases.

My other point was a matter of curiosity. In the unstabilized response of the 30-knot aircraft you have presented, could you give me a brief explanation of where the positive real root arises and possibly indicate the value of velocity stability that might have been assumed for that periodic motion?

MR. PERLMUTTER: If I may answer that question. I was involved in the stability aspect of it. We did not attempt to actually interpret your results in so far as separating any velocity stability effects or angle of attack stability effects, etc. This was not really the purpose of the investigation. In other words, the stability aspect in itself, and the nature of why does it behave in the way it behaves, was not really the reason for this research.

The basic reason for the research was to determine whether or not the slipstream itself could be utilized in some fashion so as to improve the stability of the aircraft.

CHAIRMAN CULVER: We have time for one more question.

QUESTION: [Frank A. DuWaldt, Cornell Aeronautical Laboratory, Inc.]

I would like to comment on some of the questions. I cannot recall the results, but Cumberbatch and some other people at Vehicle Research Corporation have looked at the effect of the position of the wing relative to the centerline of the slipstream — for, however, a uniform slipstream case and for the multiple jet case.

With regard to the question of whether or not one can make a very simple correction for slipstream when the wing is fully immersed, I think that the work of Graham, Lagerstrom, Licher,

and Beane at Douglas Aircraft about 1953 goes into this question, but you have to be careful to get the corrected version of 1957. It appears that whether or not the slipstream boundary condition is going to be really important depends upon the aspect ratio of the wing that's in the boundary.

CHAIRMAN CULVER: I would like to point out that one question that was asked on this paper dealt with why the equation didn't reduce to the same value as the Jones equation. It may be because the propeller was still left in; I am not sure that that was the case. I read the paper a couple of times, and that may be the problem — the propeller thrust is probably still left in.

CHAIRMAN CULVER

The next paper is titled "Aerodynamic Properties of Airfoils in Non-uniformly Sheared Flows." This paper is by W.G. Brady and G.R. Ludwig.

I think it is appropriate that they were arranged in this manner, because it does treat some of the problems that were brought up on this last paper. They are associated with the same type of problem. Undoubtedly, at least in part, it will help to clarify the case. Mr. Ludwig will present the case. Both authors are from Cornell Aeronautical Laboratory, Inc., which some of you have heard of.

Gary R. Ludwig is a Principal Aeronautical Engineer in the Applied Mechanics Department of this establishment. Mr. Ludwig received his Bachelor of Arts degree in Aeronautical Engineering from the University of Toronto in 1955 and a Doctor of Philosophy degree from the University of Toronto in 1963.

He worked with Avro Aircraft in 1957 as a Research Assistant [and was a] Lecturer at the University of Toronto from 1957 to 1961. Since that time, he has been with Cornell Aeronautical Laboratory, Inc.

His professional experience has been in the field of acoustics, aerodynamics, applied mechanics, and applied mathematics. He's been involved in investigations of flow and stall in compressors, the flow of impinging jets, and the aerodynamics of airfoils in inviscid shear flow which is the subject of the present paper. I will turn this [session] over to Dr. Ludwig.

AERODYNAMIC PROPERTIES OF AIRFOILS IN NONUNIFORMLY SHEARED FLOWS

by

W.G. BRADY and G.R. LUDWIG

Cornell Aeronautical Laboratory, Inc.

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN CULVER: Again, [please state] your name and affiliation [when asking] your questions.

QUESTION: [W. Z.] Stepniewski, Boeing-Vertol.

I am coming back to the same question I asked [on the previous paper]. From your presentation, it appears that, indeed, if you lower the thrust axis below the wing, the results should be favorable, because, first of all, you improve your stalling capability of the airfoil section and, of course, that is reflected in partial power improvement of performance, partial power of descent, deceleration, and so on.

I have another question now. About a year ago, there was some consternation when we started to think about monocyclic propellers which would introduce some shear in flow which is nonsymmetrical — nonsymmetrical shear in one direction or another. At that time, we came to Dick White and Company [Richard P. White, Jr., Cornell Aeronautical Laboratory, Inc.] asking for the inspiration [as to] whether it would be a beneficial effect or not. At that time, we came to the conclusion that it looked like the effect would be rather beneficial.

Subsequent wind tunnel tests also indicated that the difference may not be very large — almost not noticeable on small scale — but, anyhow, not detrimental. Do you have any theoretical investigation of nonsymmetrical shear?

MR. LUDWIG: Not to date, we haven't. There is no reason why the theory cannot be adopted to a nonsymmetrical sheared profile. The only requirement (to date, at least) is that we be able to approximate the velocity profile by segments such as shown there. We have not gone further than two segments. There is no need for them to be equal.

It gets more complicated, but there is no reason why this could not be extended. We hope at some time or other to be able to extend this and also to include boundaries, at least in the two-dimensional case.

QUESTION: Frank [Franklyn J.] Davenport, Boeing-Vertol.

I wonder if you would care to comment on the possibility of how relevant you feel this is, considering the fact that a real propeller slipstream is cylindrical and you are dealing with something that is two-dimensional

Furthermore, the slipstream dimensions are roughly of the same order of magnitude as the wing section that you are dealing with. This implies we have a strongly three-dimensional effect as soon as we get off the centerline. I wonder if you would comment on the differences you think this might make and if you have any plans to move in that direction?

MR. LUDWIG: We already have. The program previous to this actually investigated the force characteristics on a section of the same airfoil in an axially symmetric sheared flow which had the same distribution as that shown on the first slide. The essential conclusions reached there were that the effects are there, and some of them can be predicted strictly on a section basis provided the outer wing does not stall — that is, the slopes at the angles of attack up to 12 degrees at least have the same type of variation as was measured in the two-dimensional flow.

Of course, we can't check every point, because we only have so many two-dimensional flows. But. those that were checked appear to agree reasonably well with the two-dimensional results.

We did come across some problems after about 12 degrees . . . the outer sections of the wing stalled, and we have no way of accounting for the very large blockage effects that we attained after that. Fantastic stall angles, etc., were measured, but this doesn't mean anything.

QUESTION: [D. C.] Whittley, DeHavilland Aircraft of Canada.

I was wondering whether, really, there is any merit in normalizing the pressure distributions as a function of the stagnation pressure.

In other words, is there really any proper velocity and is it really not only an arbitrary thing to do; and, therefore, does it really have any meaning?

MR. LUDWIG: Practically, it has no meaning, primarily because you don't know what this pressure is. Conceptually, to me at least, it has a great deal of meaning — it tells me that the rather fantastic behavior that has been measured in a lot of these cases is just a misinterpretation . . . it is not a stalling phenomenon at all . . . it is just that we are using the wrong velocity.

We can break it down into two parts. Part of the misconception, at least that I had before we did a lot of this work, was that this was primarily due to stall. Well, it is not. You can get this same type of effect — the very large falling off at different locations of the lift curves — just because you are using the wrong velocity.

Now, as I said, practically, you don't know what this is, unless you have a theory (which we think we have now). Physically, at least it helps me to interpret the data, a good portion of which was just because we didn't know what the right velocity was. That is the way I interpret it.

QUESTION: Dick [Richard P.] White, Cornell Aeronautical Laboratory, Inc.

I would like to amplify on that just a little bit. For the results that Gary showed, he gave an indication that, unless you can predict the dynamic pressure — and, thus, predict the

deflection of the streamline so you can get the right stagnation streamline so you can get the right dynamic pressure . . . if I took my lift curve slope and used some other type of dynamic pressure other than the original one used — the lift which you would predict could be off as much as 65 percent. So, you have to predict the deflection of the streamline so that you can get the proper q [dynamic pressure] to get where you end up wanting to get the lift.

MR. LUDWIG: In the final case, if one were presenting the data in a form which would be more directly applicable, one would use something like a mean q or a midchord q.

However, you would still have to know what the numbers are, and it's six of one, half a dozen of the other. All I am saying is that we need the theory to get it.

MR. DAVENPORT: I gather that the procedure you used to find this critical streamline — the one that is the stagnation streamline that makes all of the data line up . . . that procedure is dependent upon being able to find the boundary between these two regions of shear. You have a computer program which needs to know the streamline shape under the influence of the airfoil. I gathered that that was also dependent on knowing the dimensions of the channel in which you were measuring. If you are talking about a real condition where you don't have those walls, you don't have this handy mass flow condition to find that streamline.

MR. LUDWIG: We feel that we can, to a certain extent, get around this by actually moving the walls out and looking for effects as we move them out. There should be a condition after we have moved them out sufficiently far — we still have our reference — in which the data we get is more applicable to a propeller slipstream.

We could, perhaps, get around it in another way also. In this particular case, it was a convenient art effect anyway to find out what the stagnation flow was. With our final velocity distribution, all we did was integrate the mass flow

from where we had to up to the airfoil and found out where that came from. Experimentally, we worked backwards, and we actually did trace where these things would have had to come from, with some very peculiar results which are presented in the report version of this paper. I will not attempt to explain what is going on now.

MR. DAVENPORT: Okay. I hope it works.

MR. LUDWIG: [Directing comment to Mr. William G. Brady.]

I think my cohort would like to make a comment on that. He was primarily responsible for the theory.

MR. BRADY: In answer to Mr. Davenport's question, from the analyst's point of view, I was very happy to have that wind wall tunnel there as a reference to compute this mass flow. I would also admit that, if you are dealing with a freestream of infinite extent, I would be at a loss for a reference.

The question really boils down in the problem of computing this mass flow [to] how accurately can you integrate a mass flow distribution over a considerable vertical distance. I can't answer that until I try it. I suspect that you might have a few problems there. How, offhand, you would go about getting around this, I can't say. But, I suspect what I would do would be to move the wind tunnel walls out a certain distance, and then hope that I still have close to freestream conditions in the computed flow.

MR. DAVENPORT: I hope so, too.

CHAIRMAN CULVER

The next paper is entitled "Experimental Investigation of Compound Helicopter Aerodynamic Interference Effects," by R.M. Segel and L.J. Bain, both of Sikorsky Aircraft, Division of United Aircraft Corporation. Mr. Bain is not with us, and the paper will be delivered by Mr. Segel.

Richard M. Segel is the Supervisor of the Advanced Rotorcraft Research Projects Group of the Aircraft Advanced Research Section of Sikorsky Aircraft.

He obtained his Bachelor of Engineering degree at Yale University in 1962, and his Master of Engineering degree at Yale in 1965.

His experience at Sikorsky Aircraft has been in conducting analytical and experimental investigations in rotary wing aircraft from 1962 to the present time.

EXPERIMENTAL INVESTIGATION OF COMPOUND HELICOPTER AERODYNAMIC INTERFERENCE EFFECTS

by

R. M. SEGEL and L. J. BAIN

Sikorsky Aircraft, Division of United Aircraft Corporation

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN CULVER: Be sure to give your name and affiliation when you are asking questions.

QUESTION: [W. Z.] Stepniewski, Boeing-Vertol.

Have you made any measurements of total power required in your investigation?

MR. SEGEL: Yes, we did measure all six components of rotor moments and forces, including torque. We found that the wing had no effect at all on the rotor power.

Some of this data is not complete. However, I am quite sure that it is consistently no measurable effect on rotor torque. There does appear to be some effect — an increase — in rotor drag due to the increasing wing lift.

MR. STEPNIEWSKI: Do you have enough data to give some prediction about equivalent lift to drag ratio of the optimum configuration, say at around 200 knots?

MR. SEGEL: I would say that this would be difficult to extrapolate to full scale because, first of all, we did have low Reynolds number, particularly on the smaller wings. Also, we had, as usual on a model, a dirty rotor head, so that the rotor head drag was equivalent for a 35,000-pound aircraft to 30 square feet. So, that hurts the equivalent L/D.

QUESTION: [A. Z.] Lemnios, Kaman Aircraft Corporation.

Do you plan on conducting any autorotation studies with your wind tunnel model?

MR. SEGEL: During this present investigation, we did operate the rotor at autorotation throughout the speed range.

MR. LEMNIOS: The fuselage and the rotor were at high angle of attack?

MR. SEGEL: [That is] right. So, of course, the wing was below stall.

MR. LEMNIOS: The wing was below stall?

MR. SEGEL: Yes, which is what you may be interested in.

QUESTION: Jan [M.] Drees, Bell Helicopter Company.

I am puzzled by the result that the presence of a wing has no significant influence on the oscillatory loads on the blades. This is contrary to what we affirm in flight tests with our high speed helicopter. I think this is due to the fact that you haven't normalized at all to the same total lift for wing plus rotor, because this is what happens if you add a wing to it — the rotor loads will go down and the oscillatory loads will go in the same way.

MR. SEGEL: Of course. The point I made was that, at a constant rotor lift — and, of course, if you put a wing on, you can reduce the rotor lift — but at the same rotor operating condition, if you put on the wing and do not change the rotor operating condition, the stresses — while theory shows they would increase — were not measured to increase. In practice, of course, the wing really benefits the rotor.

MR. DREES: The second question concerns the interference effect of the induced drag on the rotor and the wing. Have you made any attempt to correlate this with biplane theory?

MR. SEGEL: No, we haven't at this time. As I said, some of the data was only available two or three weeks ago.

MR. LEMNIOS: Thank you.

MR. SEGEL: I might add on the question of the gentleman [A. Z. Lemnios] from Kaman, we would have liked to have studied the wing at stall in autorotation, but the problem becomes, again, one of wind tunnel wall effects if we go to the large angles of attack required at the low speeds where this would be of interest.

QUESTION: Bob [Robert J.] Tracy, Naval Systems Command.

Have you had any chance to correlate your tunnel data at all with the S-61, or do you plan to in the future?

MR. SEGEL: This is something we would like to do as an in-house thing; we do not have a contract for this work. However, some of it is directly applicable; for instance, this rolling moment on the wing was experienced in the S-61F. We seem to have a decrease in wing lift on the right side.

MR. TRACY: How about any additional power required on the full scale S-61?

MR. SEGEL: I am not too familiar with the S-61F flight test results.

MR. TRACY: Thank you.

QUESTION: [W. Z. Stepniewski, Boeing-Vertol.]

In Figure 6, you show the downwash angle from the rotor on the wing and, for an advance ratio of about 0.3, if you now use typical values for C_{L_R}/σ, this is about 10 degrees or something like that [which is] quite a high downwash angle where one would expect there will be a negative load on the wing for a long time in the transition. It is not until you come to fairly high speeds that the wing will take over and it will get unloaded; is that so?

MR. SEGEL: This is true in flight test results.

MR. STEPNIEWSKI: Can you give an indication as to what speed, in a typical design, the wing would substantially come into the picture?

MR. SEGEL: I think this is pretty complicated depending on the wing incidence as well as to the fuselage and to the rotor shaft, and whether or not you have a fixed wing incidence.

MR. STEPNIEWSKI: But, it all happens very suddenly at rather high speeds — isn't that really the transfer from rotor to wing?

MR. SEGEL: I think it is more gradual in practice.

MR. STEPNIEWSKI: Thank you.

QUESTION: Frank [Franklyn J.] Harris, Boeing-Vertol.

I am trying to get a feeling again for the L/D situation. You seem to indicate that, as I go to high speed, my wing L/D will be negligibly influenced by the rotor. If I assume that I was at 250 knots and I had no lift on the rotor, and since it is an aspect-ratio-of-6 wing, I might guess that its L/D_{max} was 22. Would you say that, at 230 knots, I could use an L/D of 22 in this configurational comparison I just gave you?

I am flying along at 250 knots and the isolated wir · would have an L/D of 22. Now, I put on the rot. : — no lift in it — would I still use 22? Or would I use 21? Or . . .

MR. SEGEL: I think you probably better use 21.

CHAIRMAN CULVER: Are there any more questions?

(No response.)

I might add that I hope that the last question meant wing alone within the influence of the rotor, because, if you add on the drag of the hub and the blades and the fuselage and everything else that goes with it, I am sure the answer will not be 22.

CHAIRMAN CULVER

The next paper deals with a slightly different subject. It is STOL aircraft, which is not V/STOL, but just short take-off and landing aircraft. The paper's title is "Maximum Lift Coefficient for STOL Aircraft: A Critical Review."

This paper is by D. C. Whittley of The DeHavilland Aircraft of Canada, Ltd., and, as you know, those people do work in that field.

Mr. Whittley is a Chief Research Engineer in Aerodynamics for The DeHavilland Aircraft of Canada, Ltd. He was born in February 1921. He received his Bachelor of Science degree in Aeronautical Engineering at the London University. He emigrated to Canada from England in 1947.

His engineering apprenticeship and subsequent employment was with Saunders Roe Ltd., Cowes Isle of Wight, in aerodynamics and hydrodynamics.

From 1947 to 1949, he was a Senior Aerodynamics Engineer; from 1949 to 1956, he was a Flight Research Supervisor; from 1956 to 1961, he was Assistant Chief Aerodynamicist; and from 1961 to 1962, he was Chief Aerodynamicist. These positions were all with Avro Aircraft in Ontario; then he joined DeHavilland.

D. C. Whittley will now present his paper.

MAXIMUM LIFT COEFFICIENT FOR STOL AIRCRAFT: A CRITICAL REVIEW

by

D. C. WHITTLEY

The DeHavilland Aircraft of Canada, Ltd.

[Paper contained in Volume II of the Proceedings]

QUESTION AND ANSWER PERIOD

QUESTION: [Jeffrey P.] Jones, University of Southampton.

These calculations which you have given — these formulae for the maximum lift due to the trailing vortex system — are based on the assumption that the loading is elliptic. If you alter the calculation and don't use elliptic loading, then you don't get the same formulae at all.

The point is that the elliptic loading concentrates the trailing vorticity near the tips and, when you have a large deflection in the wake, this reduces the velocity over the airfoil, which tends to give you a reduced lift for a given circulation.

But, if you have a distributed loading, so that you no longer just decelerate the flow over the airfoil pole but accelerate it in some parts, . . . that's in the chordwise direction — for large deflections, you get quite significant changes — quite big changes. So, these formulae do stress the idea of the existence of a maximum lift due to this particular factor, but the numerical values are very sensitive to the loading distribution which you use.

The other comment [I have is:] Presumably, you would only want maximum lifts of this sort when you are near the ground; and, then, the deflection of the wake is limited. The wake just can't deform downwards. You have to take into account the fact that, at infinity behind the airfoil, it must be parallel to the ground. I wonder if you have done anything on this?

MR. WHITTLEY: In answer to the first comment: Yes, of course, it is true that span-wise loading is a very powerful effect. If one is given, say, a certain amount of C_J to blow, well, then, it is probably advantageous to concentrate this over the flat portion, provided the span-wise downwash distribution does not unload the wing tip.

We did some tests, and we have had two experiences here — I think the jet flap airplane in England has had the experience that they did a very nice job of elliptic span-wise loading, and therefore they got a very sensitive stall situation. We did the opposite; we tended to load the flap highly and had a relatively lightly blown aileron. This resulted in early root stall, and we weren't able to really fully achieve the lift potential which we think we can do. So, we feel that these are two extremes, one way or the other.

Concerning ground effect — this is really quite a knotty problem. Experience to date is mainly experimental and, personally, I haven't looked at it from a theoretical point of view.

QUESTION: [W.H.] Tanner, Bell Helicopter Company.

I [did] some jet flap work a few years ago where we got coefficients for lift very similar to what you have shown.

However, we also got drag coefficients and turning losses of such magnitudes that I was tremendously impressed by these also. Have you done any work here, especially with turning losses and losses in your ducting?

MR. WHITTLEY: You are thinking of internal ducting losses and turning losses, say, at the nozzle and external to the nozzle?

MR. TANNER: Yes, this is where I ran into a tremendous problem of fantastic losses.

MR. WHITTLEY: Yes. Well, some of you in the audience may realize that what we are working on is in fact a rather refined or complicated jet flap kind of system; then there's the wing augmenter. So, we

generate a slot behind the wing, and we actually augment the flow as it leaves the nozzle — turns over a kind of surface and is augmented in a passage at the trailing edge. We have been able to demonstrate significant increases, if you like, in thrust by this means and so, possibly, have avoided friction and turning losses over a normal jet flap kind of system.

On the question of internal duct losses, we have been able to show that these also are quite small, provided the pressure ratios you are dealing with are, say, of the order of two or more. We have been able to use work from MGTE — for instance, on right angle bends in pipes — we had 7-inch pipes and right angle bends, and we were able to demonstrate $\Delta P/q$ values of 0.2. This is very small when you consider the overall situation.

QUESTION:

[Hubert C.] Smith, Pennsylvania State University.

In Dr. [B.W.] McCormick's forthcoming book, he makes some additions to his original report. He has gathered some experimental data which verified his 1.21 times the aspect ratio for larger aspect ratios. He also recognizes [G.J.] Hancock's result of 0.885, I think it is, [and] for lower aspect ratios, it seems to agree. By low, he means 2 or below. I just wonder if you have any experimental evidence or anything to bear out anything above this in the order of 1.9 or [H.B.] Helmbold's result?

MR. WHITTLEY:

No, we really haven't. I was careful to qualify my remarks this morning. I don't think I did so in the written paper, but I think I should have done so. But, I was careful to qualify my remarks that the result of Helmbold appears to be dependent upon the rolling-up process and the fact that these vortex cores do, in fact, trail downstream in a respectable way and are $\pi b^2/4$ apart.

Now, it is quite possible that, at very high lift coefficients, this situation could change, and, if it did, then you might more closely approximate values of $C_L/A = 1.2$ (or even 0.855). From my point of view, the important thing is that what this would do would be to still further reduce

the lift coefficients which one could expect to achieve, because this corresponds to greater inclination of the lift effect of the smaller value of C_L/A. This, then, from my point of view, would make a jet flap system even more attractive.

MR. SMITH: Thank you.

QUESTION: Frank [Franklyn J.] Davenport, Boeing-Vertol.

I have an answer for your question there. There were tests conducted by NACA in 1955, I believe, of an aspect ratio 8.4, rectangular jet flap wing in which they went to very large momentum coefficients. I don't recall the TN number — Lockwood and Turner did it, I believe — but, anyway, they ran up to very high jet momentum coefficients. They did 7 and 28 and 56 — I guess they did it by running the q way down. But, when you deduct the jet thrust from their maximum lift obtained — and you come out with the so-called aerodynamic or the circulation lift, if you want — the best they obtained was 1.9 times the aspect ratio, which agrees with Helmbold's result.

Furthermore, if you plot their drag data or their total force data — if you take Helmbold's curve of induced drag versus lift — and then construct another line by drawing arcs at a distance equal to C_J from the induced drag curve, it pretty well follows the measured force characteristics of their model. I think that work substantiated Helmbold's point of view.

MR. WHITTLEY: Yes, this is true. In fact, [J.B.] Nichols, in his paper in 1957, plots out this work.

However, subsequently, both [G.J.] Hancock and [B.W.] McCormick came up with different experimental data which confirmed different theoretical . . .

MR. DAVENPORT: Well, we are looking for a maximum, and it strikes me that, if these guys have a maximum that is higher than somebody else's maximum, then . . .

MR. WHITTLEY: This is precisely why I particularly looked at Helmbold's, which did seem to be the maximum maximum — 1.9 — and studied the influence of this on sectional lift.

CHAIRMAN CULVER: I would like to see an aspect ratio of 25 or 30 sail plane wing perform by this theory. Any other questions?

QUESTION: [Jeffery P. Jones, Southampton University.]

Can I have the last word? What I really wanted to stress was that we have two, ex-university college Southampton men arguing with each other — that's really the point I want to make, the rest is unimportant.

Except that, in your paper, you make the point that the calculation on the induced drag at infinity appears as a dissipation . . . the energy dissipation appears as a translational motion. This is true, but it is only the way in which the sum is done. In fact, it assumes that this translational motion is produced by the existence of roll-up vortices.

MR. WHITTLEY: Yes, I get that. The point I tried to bring out is that, in actual fact, we know that the total energy at infinity downstream is made up of rotational components and translational components.

Now, of course, if you calculate it theoretically, you just integrate $U^2 + V^2 + W^2$ over the whole field and come out with a total theoretical energy. The thing I am rather interested in is this expression which I derived for induced drag of a jet flap. When [D.A.] Spence derived his formula — $\frac{C_{L_T}{}^2}{\pi A + 2C_J}$ — he assumed this simple momentum result that, in effect, what you say is that there is the jet, which has a certain mass, M, and a certain velocity, V_J, and at infinity downstream, it has the same translational velocity as the associated mass. Therefore, it has a certain momentum contribution and, from this balance — in a very similar way to the way I indicated in that first slide — you arrive at this expression

$$\frac{C_{L_T}{}^2}{\pi} = 2\,C_J.$$

Now, if you make an alternative assumption that, in actual fact, because M [and] V have got to be the same, the associated mass is considerably greater because the downwash velocity at infinity downstream, according to [H. B.] Helmbold, is only 1/5, so it must be five times as big. Then, you throw the MV_j term in with this, [and] then the lift contribution from MV_j is considerably less than it was, say, in [D. A.] Spence and [E. C.] Maskell's computation. If you do this, you then find that the induced drag of a jet flap is given very closely by $\frac{C_{L_T}^2}{\pi A}$. It doesn't have the $+2\,C_j$. I am just interested to know if my deliberations on this make any sense or not.

MR. JONES: We must discuss it. Thank you.

QUESTION: [Harry H.] Heyson, NASA Langley Research Center.

I think in all of this [there] is a small amourt of confusion by looking at it in terms of the angle at which the vortices are going down as being the same as the downwash angle. There is really no contradiction here. These vortices don't pass downward as fast as almost all of the mass flow. The mass flow in between them is passing downward at a much greater rate than the rolled-up vortices. For helicopter rotors, for example, and this is really nothing but a round wing — you can look at TR-1319 and see it or, for that matter, you can take a look at Peter Crimi's picture of the distortion of a rotor wake and see the same thing — the effective vorticity in the far wake is passing downward at about one-quarter of the velocity that the main mass flow has. So, there is really no contradiction at all.

MR. WHITTLEY: I wasn't suggesting that there was. When one considers momentum theory, well then you start talking about effective mass. The confusion, if any, I think arises from an effective mass which really doesn't have that much meaning. It's something which is effective overall, gross. One assigns to this, if one wishes to do so, a velocity which, when multiplied by this associated mass, gives the lift. So, one doesn't expect it to agree precisely with the exact downward velocity of the core of the vortex.

QUESTION: [William H.] Rae, University of Washington.

You people were talking earlier about having various maximum C_L maximums. We have been doing some work and we are finding out — and I think most of this data that you are talking about has come by wind tunnel testing — that there is also a limit as to how much downwash a wind tunnel can stand before recirculation will start within the wind tunnel itself. This is both a function — we're doing it with rotors — of the tunnel geometry and the size of the model to the tunnel ratio. Some other people are beginning to verify this also with jet flaps, and, if you get too high a downwash, you are getting a decrease in lift in the tunnel over what you should get. This may give you different C_L maximums based on tunnel errors, not the wing.

MR. WHITTLEY: Yes, we realize that there are experimental limitations. One has to be very careful in interpreting experimental results. In particular, for instance, both at the R.E. and at Langley, people have realized, as far as ground effect is concerned, the importance of having a moving ground.

QUESTION: [F.N.] Piasecki, Piasecki Aircraft Corporation.

For the last work on experimental verification of these various downwash assumptions, I can add a little bit. My office is in the Philadelphia airport, and my window's face the jet runway. In addition to engineering duties, I have to figure out how to pay the bills, so I spend a lot of time looking out of the window. I can tell you, from data taken from 1955 to date, that the approach of jet aircraft on a long dragging landing approach is more like Helmbold, and the take-off of jet aircraft is more like McCormick. Thank you.

MR. WHITTLEY: As far as we are concerned, I suppose, what it really amounts to is that the sectional lift characteristics are the things which predominate; and, it doesn't make all that much difference whether you take McCormick or Helmbold, but it does make a difference whether you talk about 2π or 4π.

CHAIRMAN CULVER: Well, as a brief summary, I think our papers, although they wandered around a little bit in various fields, did add a little bit to our comprehension of some of the details of the problems involved in V/STOL and STOL aircraft.

TECHNICAL SESSION V
FRIDAY MORNING, 24 JUNE 1966

SESSION CHAIRMAN
S. C. ROBERTS
Mississippi State University

Good morning. It is sure nice to see so many people turn out so early on the third day of the Symposium — it is very unusual. This is the wind-up session of the technical papers of the Symposium. It may be the last subject matter, but we think it is probably the most important critical area in aerodynamics.

We are dealing with a sort of hypothetical subject. The boundary line is really a figment of our imagination — it is just a mathematical assumption hypothesized by a gentleman called Prandtl earlier this century. Unfortunately, we have had to live with it ever since.

It is a nasty little thing, and the progress we have made in the subject of boundary layers and boundary layer control has been rather small, considering the fact that we have been working in this field for the last sixty years.

Although boundary layer is really only a mathematical assumption to aid in the solution of the general equations of motions, to the people who work in the field, it is a real, nasty, female-type piece of equipment.

Of course, the people who work in the field quite often get very emotional about it. This means, I would think, that the papers this morning should be interesting, because people who get emotional about their work can only present interesting papers. They may not be right, but at least they should be interesting.

The first paper this morning is entitled "Spanwise Flow Effects on Rotor Performance," and will be presented by Franklin D. Harris, Chief of Rotor System Technology, Vertol Division of The Boeing Company.

Mr. Harris received his Bachelor of Science degree in Aeronautical Engineering from Rensselaer Polytechnic Institute in 1956 and joined Boeing in 1956. Since that time, he has been engaged in aerodynamic research on stability and control and handling qualities for many and various rotory-wing aircraft.

In 1965, he was promoted to his present position — Chief of Rotor System Technology. He is responsible for initiation of theoretical and experimental studies of advanced rotor systems.

At this time, I would like to turn the podium over to Mr. Harris for the first paper.

SPANWISE FLOW EFFECTS ON ROTOR PERFORMANCE

by

F.D. HARRIS

Vertol Division, The Boeing Company

[Paper contained in Volume III of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN ROBERTS: I think at this time I would like to invite all sorts of questions from the floor. I would like to remind you, again, before you ask questions, would you please give your name and affiliation?

QUESTION: [A. Z.] Lemnios, Kaman Aircraft Corporation.

In your second and third slides [sic] I thought that was a rather interesting characteristic of the sweep angle — [that is what] you called it, [but] I prefer to call it sweep of lift and drag coefficient — versus angle of attack. How do you define angle of attack? Is it parallel to the freestream velocity, or is it normal to the leading edge of the airfoil? In conjunction with that, how do you define the chord for reducing your forced data to coefficient data?

MR. HARRIS: The angle of attack, as I have indicated there, is in the plane that we are all familiar with, and I hope that is normal to the span axis — a simple blade element type of angle of attack that we are referring to.

MR. LEMNIOS: Well, then, what happens when the resultant velocity vector is parallel to the span? What is your angle of attack in that case?

MR. HARRIS: Of course, our theory would say that it comes out zero then. We can't talk about it. As a matter of fact, the blade has an aspect ratio of 0.2, or

something like that. I don't think that slide pretends to show what the situation is when the yaw angle is 90 degrees, which is what I think you are saying.

MR. LEMNIOS: Well, that's right. But, I am just raising the question: Have you interpreted these curves correctly, or is there some other factor that you completely neglected here?

MR. HARRIS: No. I asked myself the same question. Is it simply a bookkeeping problem? Did I come up with a velocity the wrong way, combined with a chord the wrong way, combined with a ψ ?

I am convinced, after some severe critical review by several of my cohorts, that what I have done is very straightforward and, if you get that reference and apply the same rationale, you will do the same thing.

The reason I think it is correct — and I think it is substantiated — is that, regardless of the yaw angle, all of the data lay on a slope of roughly 0.1 per degree in this coordinate system below stall. This is, in fact, verification of the independence principle that is used very widely in the swept wing field. They will throw it over and work out their wing elements normal to the span axis, and they will assume that C_L is 0.1 per degree, 5.73 per degree . . . and that angle remained the same with all of the data reduction. What happened was that the C_L maximums went up.

QUESTION: [Harold A.] Cheilek, Cornell Aeronautical Laboratory, Inc.

First, I would like to congratulate the speaker on a very fine discussion of the unsteady two-dimensional effects, which are long overdue as far as being applied to the rotor design theory.

However, I would like to point out some limitation, at least as far as you carry the argument in reasoning, from a sinusoidal test to the rotor case. In the sinusoidal test, the αdt_0 or the $\dot{\alpha}$ were equal both on the increasing and decreasing side. Therefore, the hysteresis loop

did, in effect, balance out to roughly the stall value of C_L. If you were to use that in the rotor case, you would find no change in the average total lift. However, in the actual inflow on a rotor, the $\dot{\alpha}$'s are not equal — probably by large factors on the increasing and decreasing side. So, it is quite conceivable that you could come up with a higher total lift, and that should not be overlooked.

MR. HARRIS: I think I understand your point, and it is very well taken.

CHAIRMAN ROBERTS: Well, it doesn't look as if there are any more questions from the floor. It must have been a very lucid presentation. Thank you.

CHAIRMAN ROBERTS

The second paper of the morning shall be presented by Dr. Henry R. Velkoff, and the title is "A Preliminary Study of the Effect of a Radial Pressure Gradient on the Boundary Layer of a Rotor Blade."

Dr. Velkoff received his Bachelor of Science degree from Purdue, Master of Science degree from Ohio State University, and Doctor of Philosophy degree from Ohio State University. He has had experience in many places including Lockheed Aircraft Corporation, Aerotor Associates, NACA, Langley, and Wright Field. He has a list of experience that goes on forever.

His publications cover pages, and they are many and are varied, and he has had many honors, especially for his work in electrostatic fluid interactions.

At this time I would like to present Dr. Velkoff.

A PRELIMINARY STUDY OF A RADIAL PRESSURE GRADIENT ON THE BOUNDARY LAYER OF A ROTOR BLADE

by

H. R. VELKOFF

Ohio State University

[Paper contained in Volume III of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN ROBERTS: Thank you for a very clear definition of three-dimensional boundary layers, and I hope it gives us food for thought. At this time I would like to open the floor to discussion of the subject.

QUESTION: [Jeffrey P.] Jones, University of Southampton.

Can I ask you [to explain] the significance of the hyperbolic differential equation that you got? You used the method of characteristics. What does this imply about the propagation? What is propagating, and what does this imply?

MR. VELKOFF: I am not certain that I viewed it in that fashion, although I restudied all the methodic characteristics, obviously, quite thoroughly in trying to solve the equation.

As far as what is propagating, I honestly could not answer that question. I tried first to transform the equation by Laplace. And, handling it that way, we got solutions; but they were so complex that, when you were all through, you couldn't interpret too much.

Actually, we also have solutions for various others. For example, the simple yawed flow that Mr. Harris was talking about — where we had a constant velocity, and that is a different characteristic. If you have a constant linear variation squared — and other possibilities — you get quite different characteristics incidentally for your solutions.

Thus, this particular one I demonstrated — I'll be real honest — I showed this one because it is simple — you can put it on there. But, if you use some of these others, it is lots and lots of series expansions which are very difficult to interpret, and, without test data to go back to and tie this thing together, I don't think you can draw many conclusions.

QUESTION: [Speaker did not identify himself.]

I think this is fair, because it is just a comment. The problem that you presented there should be viewed, I think, by the audience as darn near overwhelming, and yet the situation that is shown is a laminar boundary layer, and Hank is quite correct in saying that he is able to identify a span-wise equation to work with and then a chord-wise equation to work with. But, the whole swept wing aerodynamics field is plagued with an argument right now [as to] whether that kind of assumption could be used if the boundary layer were turbulent; that is, could you split the boundary layer or Navier Stokes equations into two identifiable equations, one describing the boundary layer of growth span-wise and one chord-wise. They are at real loggerheads, and it makes a very difficult problem just to throw in the turbulent shear. So, admittedly, the vortex problem in potential flow is an interesting one to work on, but let's bring some of our power to bear on a problem like this.

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

I just wanted to congratulate you, Hank, on this most provocative presentation on a problem I think is most timely.

I wanted to add just a piece of information regarding some propeller tests that we had made some time back which perhaps may have escaped your attention. They were strictly experimental, and they [consisted of] stall flutter investigation of a quasi-supersonic type propeller [in] which we had a thrust rake behind the propeller measuring the sectional lift coefficients, in essence.

We discovered, as you have indicated, that there must have been a span-wise gradient in the boundary layer, because, in the inboard sections, we were able to obtain lift coefficients far in excess of what two-dimensional characteristics would have indicated; while in the outboard sections, we were experiencing premature reduction in the lift curve slope. I would not say it was stall . . . we didn't pursue these to any degree at the time, because this was another kind of investigation. But, it certainly would be interesting to go back to this uniform case again to study this. Perhaps it might shed light on a nonuniform case which you are considering at the present time.

MR. VELKOFF: Was this ever reported in any detail?

MR. YAGGY: The [report] number escapes me.

QUESTION: Jack [Anton J.] Landgrebe, United Aircraft Research Laboratories.

I have a comment concerning the two papers this morning. It concerns making this problem sound even more complicated, as far as the skew effects and the boundary layer effects. We don't [want to] lose sight of the induced effects in coming up with the equilibrium or to get the actual flow of a particle along the blade.

In respect to what we learned yesterday, it is possible that these vortices coming off the blades can actually pass very closely to — let's say, the following blades, especially in the regime we are talking about, which is up towards the higher speeds where the wake skew angle can be fairly shallow.

If this occurs, and you have vortices coming very close to the points on your blade, it is possible you get induced flow span-wise in all directions, which could add to your centrifugal flow and your coriolis and everything else. This just tends to complicate the problem further.

MR. VELKOFF: I agree. The in-flow studies that have been sparked in the last four or five years, at least to me, are probably the most significant thing

that has happened to the rotary wing business since some fellows in a club up here at Buffalo, by the name of Cornell Aero, stumbled across something we called the effect of higher mode bending of resonance effects or something. But, although I think the primary effort that everybody should put — I am biased, I will admit — is in this in-flow work.

I strongly feel we also have to bring the boundary layer work somewhere up close. Now, since I have the microphone and a captive audience . . .

When you look at the literature in boundary layer work — and whether it is the chemical engineers and their mass transfer problems or heat transfer problems, the mechanical engineers, the magnetohydrodynamicists, the classical aerodynamicist . . . hundreds of people and thousands of papers are written on boundary layer work — the look-see at what the boundary layer is on the rotor was fairly well limited, and it was with [W.R.] Sears at Cornell University and his men, but they looked at a restrictive case of the hovering case with certain limiting conditions which, if you look at the theory, the hovering mode should not have any real strong coupling effects.

The area is wide open. It may turn out there is nothing there. I don't think we can afford to . . . it's just like we said on that in-flow: It is a tough problem; we'll stay away from it. The same thing is true here. I don't think we can afford to — I think somebody has to get into it.

QUESTION: [A. Z.] Lemnios, Kaman Aircraft Corporation.

I would just like to comment on some unpublished test results that I obtained a few years back on a model rotor. I think this is a fairly common phenomenon, that most people have been up against, of obtaining c_D versus data curves which apparently don't really come up with any kind of a stall characteristic. You don't really see any stall angle, and, in order to try to find out what happened here, I tried putting a boundary layer fence on the rotor, approximately at the 50 percent radius — it was essentially an elliptical shaped boundary layer fence and [it was] a good size one. It didn't seem to indicate any difference whatsoever. I just throw this in for added food for thought.

MR. VELKOFF: Thank you.

CHAIRMAN ROBERTS

The third paper of this morning shall be presented by Mr. Watson H. Tanner and Mr. Paul Buettiker. The title of the paper is "The Boundary Layer of the Hovering Rotor."

Mr. Tanner received his Bachelor of Science degree from the University of Bridgeport in 1959. From 1957 to 1965, he worked at Sikorsky Aircraft, Division of United Aircraft Corporation. From 1965 to the present day, he has been with the Bell Helicopter Company.

He is an aerodynamicist, and is primarily responsible for the aerodynamic research including what you will hear about this morning.

Paul Buettiker is an assistant professor of the aerospace engineering department of Arlington State College. He received a Master of Science degree from the Swiss Federal Institute of Technology in 1957, and he is getting his Ph. D. from Tulane University in New Orleans.

From 1957 to 1959, he worked at Contraves in Zurick, at Brown University, and at Tulane University. From 1964 to present, he has been at Arlington State College.

Unfortunately Mr. Buettiker is not able to be present this morning, but Mr. Tanner has decided to give the paper without the moral support of his co-author.

THE BOUNDARY LAYER OF THE HOVERING ROTOR

by

W.H. TANNER

Bell Helicopter Company

and

P. BUETTIKER

Arlington State College

[Paper contained in Volume III of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN ROBERTS: Thank you Mr. Tanner. At this time, I would like to invite discussion from the floor.

QUESTION: Frank [Franklyn J.] Davenport, Boeing-Vertol.

I thought it was a very good paper, Hank. I especially feel that the work you have done with the hot wire on the rotating blade in forward flight is a sign that there is a great big hill full of gold to be mined in terms of developing our understanding.

I do want to comment on two things. The question of using a laminar flow profile has to be looked at carefully, because, in forward flight, you are going to have large excursions in α, and profiles designed to maintain laminar flow in the bucket region tend to go to pot fast when they are at higher C_L's than the bucket. That could cause a great deal of grief.

Second, I don't really think it was fair to say that we aren't including this inboard moving vortex in our hover theory. I think Mr. Erickson's paper really showed that — well, there you have a contracting and accelerating wake and he had vorticity inboard of the tip there, and I bet if he had shown a span-wise loading, he would have had the same phenomenon. Of course, he was talking about propellers and much higher C_T's than were customarily used on a helicopter rotor. But, if he had applied that same theory to the low solidity

of a helicopter rotor, I think you would have seen the same phenomenon that you point out there in that theory.

MR. TANNER:

I agree with you on both points. What I meant by the second — I should have said I haven't included this. What you say about this . . . I am sure, if he uses this — except for the solidity and higher rpm — they should get the same solution from the propeller, certainly.

Your other comment on the laminar flow profiles . . . all I can say there is that you are right. But, I think they offer such promise that we no longer can put them on the shelf and neglect them. I think we must re-examine and take a serious look at the use of laminar flow profiles again; because, better ones are being made today than were made in the past and better theories for designing them are available.

MR. DAVENPORT:

I know that is true.

QUESTION:

[D.H.] Henshaw, The DeHavilland Aircraft of Canada.

I was very interested in your remarks on the laminar flow airfoils. I wondered if this test airfoil was specially made to avoid waviness in the surface? I believe that, at higher test Reynolds numbers, there may be problems with premature separation if one has slightly wavy surface.

MR. TANNER:

The answer to that is that we made two blades — one very good and one very bad as far as waviness goes. After we painted these blades, we found [that] we got a distinct waviness in one of them, because the grain raised. But, it was just grain raising . . . a thousandth, or something like this, is what we are talking about, on that order of dimension.

For this particular profile — I have seen this with other laminar profiles — this did not separate. We couldn't tell the difference between what we call the good and the bad. Where with other profiles, I have seen just a small bump here make the flow go completely turbulent.

QUESTION: [Paul F.] Yaggy, Army Aeronautical Activity.

I want to call to the attention of Hank — he realizes this, but I want to point this out — that we have made tests on this same laminar section in our wind tunnel since these results which he's reporting in the paper. We've done this at skew angles from 30 degrees to 150 degrees and angles of attack from 0 to 6 degrees. We have shown that the effect of the skew angle does not change the length of the laminar run; that is, we still maintain an approximately 50 percent laminar run on the upper surface. The only difference we seem to see at the moment — and our results are preliminary, so we only comment on this briefly — is that the lower surface seems to demonstrate 100 percent laminar run at a slightly higher angle of attack than was indicated in the rotating test. But, we feel that [it] is very significant that you can maintain these laminar runs through these high skew angles.

MR. TANNER: Thank you.

QUESTION: [Aldo] Peracchio, Hamilton Standard.

I was wondering if you were able to determine the strength of the vortex right at the tip, especially in comparison, say, to the strength of the tip vortex one pitch below the blade. Were you able to get any feel for that?

MR. TANNER: No. We didn't even experimentally attempt it. Our theoretical analysis is based on potential theory, not [on] real vortex theory, which you would need to do this analytically at the tip. We were working on this, but it has not been done.

QUESTION: Jack [Anton J.] Landgrebe, United Aircraft Research Laboratory.

Concerning Figure 15 [page 33 in prepared paper], where you show the induced velocity versus radius — that is where you showed the location of the vortex underneath the blade — how far below the rotor disk was this vortex located, and how did you determine this?

MR. TANNER: I am trying to think of the number there, Jack, and it completely escapes me. I think it was on the order of 2 to 2.5 feet. I can't say for sure; I may be wrong. How we got the vortex spacing was, we scaled a wake from smoke flow pictures and, if you know the strength of the vortex and the wake shape, you can then iterate on a solution — but, you must know the wake shape; that is the gimmick so this will work. You can then iterate on a solution which spaces the vortices — and we used 20 of them in this analysis — below the rotor at the proper distance to give you the desired distribution back at the disc. This is the way we did it.

MR. LANDGREBE: This was all analytical then?

MR. TANNER: Yes, this was strictly analytical. However, it corresponded perfectly with our experimental results.

MR. LANDGREBE: When you mentioned that the location of that vortex was directly below the peak on the induced velocity curve, were you implying this is what was causing the peak — this vortex below the blade?

MR. TANNER: Yes.

MR. LANDGREBE: I think that what you will find is that, rather than that vortex causing that particular peak, I think you can explain it by taking . . . well, first of all, that vortex would not really induce a velocity in the axial direction if it is directly below the point. It would only induce a velocity radially, because the induced effect is circumferential.

I think what you are seeing here is [that] the effect of the tip vortex of the blade itself is going to induce an in-flow which would give you, normally, this increased in-flow curve that we start to see here towards the tip. This vortex that is directly below the point will tend to give an upflow outboard of it, which would explain the other peak — the peak around 95 percent — rather than the peak that is directly over it.

MR. TANNER: I am a little confused by what you said, Jack. I disagree with you though.

MR. LANDGREBE: If you place a vortex directly below a point, the induced velocity due to that vortex is going to be in the circumferential direction, which means it is going to be along the blade span-wise rather than axially.

MR. TANNER: You get components in both directions.

MR. LANDGREBE: No . . .

MR. TANNER: [Directing comment to Jan M. Drees:]

Maybe he's got a better handle on your specific question than I have.

MR. DREES: What I recall from our calculations is that the main effect is being caused by the, say, 18 vortices that are down below. The quarter vortex that comes off the blade is very powerful very close to the blade. Then, the second vortex is quite powerful, but, then, the sum of all of the vortices that are down below — [or are] positioned at about the $\sqrt{2R}$ — add up to a very sizeable effect. I believe that the place of the dip is being determined by those vortices. It happens to be that the second vortex is just down below that dip. At least, that is the way I interpreted the data.

MR. LANDGREBE: My only contention is that this vortex that is below will create an upflow outboard of it, which will give the dip that you see around 95 percent — not the actual peak that is directly above it. I think what it is doing is . . . well, the tip vortex of the blade itself will give you, if you had that alone, a curve that looks like this [speaker motioned with hands].

Now, what is happening is that vortex that is directly under the blade at 80 percent is creating an upflow between 80 percent and the tip, which is giving you this dip that you see, rather than the actual peak.

CHAIRMAN ROBERTS: Gentlemen. May I suggest that you continue this argument outside. I think your time is up on this subject. Thank you.

CHAIRMAN ROBERTS

The last paper of the session is entitled, "An Investigation of the Feasibility of a Common Boundary Layer Control System for High-Lift and Low-Drag on an Airfoil Section," and is to be presented by Séan C. Roberts.

If I can take off my hat as Chairman and put on another hat as speaker, I will carry on from there.

AN INVESTIGATION OF THE FEASIBILITY OF A COMMON BOUNDARY LAYER CONTROL SYSTEM FOR HIGH-LIFT AND LOW-DRAG ON AN AIRFOIL SECTION

by

S. C. ROBERTS

Mississippi State University

[Paper contained in Volume III of the Proceedings]

QUESTION AND ANSWER PERIOD

CHAIRMAN ROBERTS: As Chairman, I would like to open the floor for discussion. You can't ask the Chairman any questions, but you can the speaker.

QUESTION: [Henry R.] Velkoff, Ohio State University.

Your tentative conclusions of an alternate to the laminar suction approaches by using the pressure seems to lead to some of the older conclusions in this work where, if you use pressure, might you not just stagger the slots backwards where we were somewhere [around] Prandtl — certainly in the early 1930s and 1940s. Is this reasonable? If you got the energy anyway, is there any advantage of directing it slightly aft rather than simply out like a normal transpiration system?

MR. ROBERTS: No. We did some experiments on angle slot transpiration where we angled the slot from 0 to 180 degrees, and, for reducing the drag, we did find that angling it forward about 30 degrees was probably the optimum. Trying to get any reduction by sending it the other way . . . we were not impressed by that at all. In fact, slot blowing was not very good because of the tremendous losses in the very small slots that is required.

QUESTION: [Harold A.] Cheilek, Cornell Aeronautical Laboratory, Inc.

This is more of a question from ignorance, but, if I recall my reading in Aviation Week, I think Northrup has had a very large program for the cruise condition. I am not sure if you refer to this program at all, and if not, could you elucidate what progress they have been making and how it reflects on your talk?

MR. ROBERTS: Well, the Northrup business has been a laminar low drive boundary layer control system with converted B-66's, and used distributed suction through very small slots on a wing to stabilize the boundary layer. I am suggesting that [that] is fine as a very large, expensive project, and its success has been sort of marginal to date.

I don't think that you can use a laminar [boundary layer control] system for a high lift aircraft that is going to operate in and out of places that you cannot maintain cleanliness — which, in the laminar BLC, [we] feel is next to godliness — and you can't have smoothness, and you can't have all sorts of things. I am suggesting an alternative system to the laminar BLC system.

QUESTION: Frank [D.] Harris, Boeing-Vertol.

I wonder if you could comment [on] how you see this kind of system working on a helicopter blade. Profile power and power given up to skin friction in our system is just as important to us. If we could get rid of it or make it negative, as you suggested a trend . . . that sounds great.

MR. ROBERTS: I thought that, with all of these helicopter people, it was only a matter of time before someone would ask me that question, and it is a rather difficult one to answer. Rotor blades are generally very, very small, and it is difficult to put BLC systems in them with any degree of success. I am not so sure that this is not possible with the larger helicopters; however, it seems from the results [presented] early this morning that you have a large degree of laminar flow anyway — without the BLC system. So, perhaps it would be better just to leave it as it is. Be thankful for what you have.

Well, if we have no further questions, I would like to resort to the Chairmanship.

The summary of the session seems to be that we have one paper that says we are going to have outflow and one that says we are going to have in-flow on the rotating blade, and that just falls into the general discussion normally associated with boundary layers. One year, we [use suction] for BLC's, [while] the next year [blowing appears more advantageous].

It all seems to resort to the one fact that we don't really know. The mathematical model of the boundary layer equations is very complex, and it has not been solved with any degree of success for the practical cases.

However, with the use of the new computers, high speed and analog, the solution of these equations should probably take up less time of the aerodynamicist, and, therefore, he should have more time to spend in finding out the actual flow in the two- and three-dimensional boundary layers and ignore the pressure gradient with transgression.

TECHNICAL SESSION VI
FRIDAY AFTERNOON, 24 JULY 1966

RESEARCH REQUIREMENTS AS RELATED TO THE PREDICTION OF V/STOL AERODYNAMIC CHARACTERISTICS

INTRODUCTION

Technical Session VI consists of two Panels, including prepared papers and attendant question and answer periods. Pages 167 through 290 contain the prepared papers of the members of Panel I, with its corresponding question and answer period following. A summary of the Panel I discussions, as prepared by Chairman Hewin, is presented on pages 299 through 300. Pages 303 through 360 contain the prepared papers of Panel II, its corresponding question and answer period following. A summary of the Panel II discussions, as prepared by Chairman Cheilek, is presented on pages 371 through 372.

The members of the Panels and their respective affiliations are listed on the pages preceding the prepared papers.

We are indebted to the Panel members for furnishing their prepared papers in a form that could be directly reproduced. This material was published as provided by the authors and was neither checked nor edited by CAL or USAAVLABS.

Technical Session VI

Panel I

Chairman

LARRY M. HEWIN, Technical Director

U.S. Army Aviation Materiel Laboratories

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AERONAUTICAL RESEARCH REQUIREMENTS
AS DETERMINED FROM THE X-19 AND X-100 VTOL PROGRAMS

by

H. V. BORST

SUMMARY

From the experience and data gained during the development and test programs with the X-19 and X-100 tilt propeller airplanes, it became evident that many areas of aerodynamic research are necessary to assure the success of future VTOL aircraft. The research areas suggested apply to conventional single wing tilt propeller or tilt wing propeller type VTOL aircraft. The technology required occurs mainly in the areas involving improvement in performance or stability and control and do not involve fundamental problems which would deter building and operating of vehicles of this type.

The following areas warrant additional aeronautical research:

1. Investigate methods of reducing and predicting propeller and rotor download losses due to wings and bodies

operating in the wake.

2. Determine proper techniques for conducting model wind tunnel tests at conditions during conversion to account for problems associated with Reynolds number effects.

3. Conduct theoretical and test programs for finding the effects of local velocity changes on propeller flutter derivatives.

4. Investigate influences wing-propeller parameters on translational lift characteristics to determine reasons for differences in model and full scale performance.

5. Obtain further data on hinge moments of control surfaces as used on thick wings in the 18 to 21 percent thickness ratio range.

6. Investigate descent rate limits encountered with propellers operating at disk loadings in the 15 to 50 psf area.

7. Determine the effects on stability and control characteristics in conventional flight of the use of large rotors or propellers.

INTRODUCTION

The X-19 VTOL airplane program was undertaken to evaluate the characteristics of the tandem wing tilt propeller approach and to determine the operational suitability of vehicles

of this type. Two aircraft were built for the program. The first airplane was to be used for aerodynamic performance, stability and control testing whereas the second vehicle was to be used for structural flight testing. The flight test program started with hover testing and then proceeded to low speed conversion tests leading to full conversion. Unfortunately, after 50 flights, the number one X-19 was lost due to a failure of a nacelle which caused the loss of a propeller and the ultimate crash of the airplane. This failure, which occurred at a speed of about 110 knots, was caused by a resonant blade condition and a stress concentration within the nacelle. These problems are not associated with the tilt propeller idea and can be avoided by proper design changes. These design changes have been incorporated in the second aircraft.

Although the original flight test program of the X-19 has been terminated due to lack of research funds, consideration is being given to the testing of the second airplane in the Ames 40 x 80 foot wind tunnel. A tunnel program would answer many questions on the stability, control and performance of tandem wing aircraft. While all the answers hoped for will not be obtained in the wind tunnel, the test is believed to be desirable and should be undertaken.

Associated with the development of the X-19 was the design and test of the X-100 research VTOL airplane. This aircraft was a small tilt propeller type using only two propellers mounted at the tips of fixed wings. The planned flight test program was successfully completed with the X-100 airplane and many important advantages of the tilt propeller airplane were demonstrated. Further details on the X-100 and X-19 program are available. [1-3]

Although an extensive effort was expended in the course of the development of the X-19 and X-100 and a considerable amount of model and full scale testing accomplished, further research is required if operational tilt propeller aircraft are to be designed and built. In describing the areas where further aerodynamic research is required, only those areas pertinent to tilt propeller single wing type are considered. Many of the items are important whether the wing is fixed or tilts with the propeller.

Although a large amount of research was accomplished on the problems associated with tandem wing aircraft, there still are many areas where further aerodynamic research is necessary for the continued development of the tandem wing type VTOL

airplane. It is felt that these research items should be held up until it is apparent from the wind tunnel testing of the X-19 whether tandem wing aircraft should be pursued.

Research on propeller operating at the hover condition will not be discussed as this problem, although not completely solved, is being actively pursued and in due course should be satisfactorily solved.

RESEARCH REQUIREMENTS

To design a fixed wing tilt propeller configuration for VTOL aircaft that will give peak performance along with satisfactory stability, control and aeroelastic characteristics at all flight conditions requires additional aeronautical research. The areas of importance are associated with the combination of the wing, nacelle, propeller and fuselage, and are encountered because the wing is small in comparison to the propeller, and the propeller will operate through a large angle of attack range. Also, the wake of the propeller influences the performance of the wing and this wake varies with nacelle angle, speed and power. The relative size of the wing and propellers used on VTOL aircraft is quite different from conventional airplanes as the propeller is sized to provide lift equal to weight at take-

off and the wing can be sized for peak performance at the cruise flight conditions. The relative wing to propeller size is illustrated by noting the ratio of disk area to wing panel area is 4.75 on the front wing of the X-19 and 2.65 on the rear wing. On conventional aircraft the ratio is less than one.

For wings and propellers of the relative size and type discussed above, aeronautical research is required as described in the following paragraphs.

Download Investigation at Hover

The decision on the use of a fixed or a tilt wing for a VTOL airplane with propellers depends to a large degree on the magnitude of the download loss between the propeller and wing. If the download loss can be held to a reasonable value with fixed wings, it may prove desirable to tilt only the propellers. This could be the case as lower disk loadings can be used with fixed wings without danger of wing stall during conversion and due to lower disk loadings, higher values of lift to power ratios are obtained including the download loss. Weight savings may also be obtained in the structure of fixed wing aircraft which will make possible improved payload weight relationships.

On the X-19 aircraft the hover losses due to download on

the wings was of the order of 9 to 10% of the propeller thrust measured in free air. This loss was with flaps set at approximately 60 degrees. If the flaps were at zero degrees, the loss would be 15 to 18%. Based on the q in the slipstream, as determined by momentum consideration, the drag coefficient of the wing with zero flaps is 1.1. The drag coefficient with flaps deflected is .6 to .7 still based on the wing area at zero flap. A limited amount of testing indicated that the drag coefficient could be reduced to .3 to .4 by a slot type of arrangement at the leading edge of the wing. It is believed that further development can reduce the losses to still lower values and so improve the overall performance of tilt propeller aircraft. The program should include the effects of disk loading, shaft to wing chord angle, wing area immersed in the slipstream, variation with changes of forward velocity, and a large variation of lead edge slots, flap configuration and other devices useful in reducing the drag. A research and development program of this type should net drag reductions of sufficient magnitude so that large improvement of payload can be obtained with the fixed wing tilt propeller VTOL airplane.

Translational Lift Characteristics

The initial wind tunnel testing indicated that the STOL

characteristics of fixed wing tilt propeller VTOL aircraft would be poor. These results were based on single wing and propeller tests.

With both the X-100 and X-19 aircraft a reduction in power from that required for hover was obtained with increases in forward speed. This power reduction was obtained due to the translational lift characteristic of the propellers and the elimination of the download losses on the wing. The minimum power for the X-100 and X-19 is 30 and 40% respectively of that required for hover and thus good STOL characteristics and overload performance are shown for tilt propeller fixed wing types. Since the wind tunnel testing did not predict this characteristic, a research effort is necessary to clarify and determine the magnitude of the effect. Large scale testing of several propeller wing combinations are required to properly investigate the variation of lift and power due to translational speed. The testing should be done for various combination of wing to disk loadings and should cover a range of operating speeds.

Low Speed Wind Tunnel Testing

The performance, stability and control characteristics of VTOL aircraft at conditions corresponding to the conversion

region from hover to conventional flight are generally the most critical and, therefore, accurate wind tunnel test data are required in this low speed operating range. When VTOL aircraft models with propellers are tested in the critical conversion region, it is necessary to duplicate the advance ratio and thrust loading so that the flow condition over the wing and, hopefully, the propeller moments will be properly simulated. During this low speed testing power is used to directly generate lift and this has led to concern over the effects of the tunnel wall on the results. To overcome this problem, large tunnel sections have been used to minimize the wall effects.

Unfortunately, the performance characteristics of propellers is effected to a large degree by operating Reynolds number so that although the thrust loading may be duplicated, the power loading and moments are not the same as those obtained from full scale tests. Thus, even if wind tunnel wall corrections are eliminated by the use of large tunnel sections or the equivalent, the effects of Reynolds number encountered by the use of model propellers must be considered and corrections developed to convert the model data to full scale.

The combination of low operating speeds and relatively

small test models also leads to Reynolds number problems associated with the measurement of the performance, stability and control characteristics of the airplane. For instance, the measurements of the performance of the VTOL models operating at high nacelle angles can be influenced to a large degree by Reynolds number effects as the model nacelles will be operating at Reynolds number well below the critical with the result that the drag will be much higher than would be obtained full scale.

Since wind tunnel test data is of vital importance in the development of VTOL aircraft, research is suggested to determine the most suitable techniques for model testing and converting the data to full scale results. Effort is particularly needed in the conversion range between hover and conventional flight.

Propeller Aerodynamic Effects in Flutter

The possibility of a propeller installation becoming involved in a flutter-type instability has been known for many years, and was, in fact, pointed out as early as 1938. This phenomonon, which is commonly known as whirl flutter in its pure form, did not receive much attention until 1961, but since then several investigations have appeared. Recently, require-

ments to investigate propeller effects in the flutter analysis of conventional aircraft were made part of the Civil Air Regulations.

In the case of VTOL aircraft, such as the X-19, the relatively large size of the propellers compared to the wings makes the aerodynamic effects of the propellers very important in the flutter characteristics of the aircraft. Various other unconventional characteristics and features result from the design of the X-19 as a VTOL aircraft. These do not, however, necessarily degrade the flutter characteristics of the aircraft, but do suggest a cautious approach to the application of previous results intended for conventional aircraft. Thus, in order to detect any potential flutter modes peculiar to VTOL airplanes, such as the X-19 configuration, it is considered necessary to include the aerodynamic effects of all the rigid propeller motions, except for perturbations in its rotational speed. The result of this provision is the requirement for propeller load and moment derivatives with respect to displacement and velocity in each of the motions. This requirement made it necessary to supply as many as 15 distinct derivatives, even with certain simplifying assumptions to conduct the analysis. The operation of the propeller at large angles of attack did not

permit the use of the symmetry conditions so useful in similar previous work. The large angles of attack also result in intermittent stalling on some blade sections, invalidating the usual linear relationship between section lift coefficient and angle of attack.

The existing theoretical treatments calculating the propeller stability derivatives, Houbolt and Reed[4] and H. S. Ribner[5] do not include the effects small but finite relative velocity and angular perturbations which might be induced due to structural vibrations. Thus the propeller derivatives determined by these theories must be used with caution as second order effects are not included.

It is recognized that to calculate the necessary propeller derivatives for VTOL aircraft flutter analysis and include the secondary effects is a major undertaking. But, it is believed that the propeller will react to these changes and, therefore, a program is believed necessary. Before a theoretical analysis of the problem is attempted, tests should be made to measure the propeller derivatives under dynamic conditions simulating the second order dynamic effects. The results of these measurements should then be compared to the measurements

under static condition and from this comparison, determine if the existing theories are adequate.

Hinge Moments - Thick Wings

On some VTOL aircraft it may be desirable to use thick wings to provide an adequate structure for the propellers or rotors mounted at the wing tip. The use of control surfaces on the thick wings demands knowledge on the design of the juncture between the flap and wing and the magnitude of the hinge moment. Considerable data is available for wings with conventional sections but little data is available for wings in 18 to 21 percent thickness ratio range as were used on the X-19.

Descent Rate

There has been a considerable effort expended to improve the rate of descent of tilt wing aircraft when operating low values forward speed. This effort has been directly expended to increase the wing stall angle and, thus, the rate of descent. Little effort has been expended to determine the characteristics of the propeller or rotor when operating in this region. It is known the rotors and propellers can enter the vortex ring state with the resulting high levels of vibration and destabilizing flow problems. Tests should be conducted to pinpoint the

boundaries of the vortex ring state as a function of forward speed, rate of descent and disk loading. These tests should be conducted for full scale propellers so that the effects of Reynolds number are eliminated.

Stability and Control

Because of the relatively large diameters employed, propeller or rotor driven V/STOL aircraft operating in the cruise configuration tend to have values of the side force due-to-sideslip derivative (Cy_β) which are two or three times the magnitude of this derivative for "conventional" aircraft. The consequences of this large Cy_β is to increase the lateral "g" response of the V/STOL machines to: (1) lateral gusts, and (2) to any pilot uncoordination during roll tracking maneuvers. The first of these effects is rather self-explanatory. The second, however, may merit some comment. For example, if one considers that any given pilot is somewhat uncoordinated in his stick and rudder inputs, then, he will experience sideslip errors and an attendant lateral "g" response in flying a V/STOL which is two or three times that which he would experience in a "conventional" airplane. With this fixed human error, and with the propeller or rotor diameter fixed for several performance reasons, the airplane designer is constrained in his approach toward

reducing lateral "g" response.

Lateral "g" response to gusts can be alleviated to some extent by careful attention to blade geometry so as to minimize the C_{Y_β} contribution of the propeller at the more critical conditions. Also, attempts should be made to minimize vertical tail C_{Y_β}.

Response due to uncoordinated lateral control can be minimized by careful attention to control sensitivity (particularly rudder) and control harmony. Some form of non-linear rudder gearing can be very powerful in this respect. Furthermore, strict attention should be given toward reducing any roll-yaw coupling to decrease the amount of control activity required.

In regard to this latter statement, it should also be recognized that propeller or rotor driven V/STOL aircraft also tend to have substantial dihedral effect due to the high C_{Y_β} coupled with what is generally a significant vertical displacement of the propeller from the airplane center of gravity. This in turn necessitates larger-than-normal vertical tail volumes for good Dutch Roll characteristics, and for reasons of weight and drag this tail volume is usually attained with a large vertical tail having its a.c. well above the c.g. which further

aggravates an already large C_{l_β}. Obviously, the designer must conduct trade off studies to see if twin vertical tails with their low c.p. locations lead to significant improvement.

It is therefore recommended that further research be conducted to evaluate stability and control problems involved due to the use of large propellers operating in the cruise mode as used on VTOL aircraft.

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THOUGHTS ON PROGRESS IN ROTATING-WING AERODYNAMICS

by

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INTRODUCTION

As a framework for discussing where we are and what needs doing about aerodynamics of helicopter-type rotors, it is convenient to think in terms of current problems as seen by the user, and that approach is used in this paper. One well-recognized problem is the need for increased maneuver capability at cruise speed; this one leads into several sticky aspects of basic rotor aerodynamics. A second is cruise efficiency; in conjunction with maneuver capability, this problem leads into the question of aerodynamic redesign of rotors. A third problem, or actually a convenient grouping of problems, is noise, blade stress, and vibration; this grouping was chosen to lead into discussion of what might be done to reduce the unwanted effects of blade-tip vortices. Following discussion of these problems, a few tentative thoughts on getting faster progress virtually without added cost are given.

MANEUVER CAPABILITY AT CRUISE SPEEDS

Aerodynamic Anomalies

High accelerations at cruise speeds, by requiring high thrust when the retreating blade is already at a high angle of attack, leads straight to the question of retreating-blade section characteristics and boundary-layer

behavior at high angles of attack. Here we are faced with a number of anomalies. For example, studies of referen-- 1 as well as several subsequent studies have indicated predominantly forward flow on the blade surface after high-angle flow separation, while other studies such as that of reference 2 have shown predominantly spanwise, outward flow. As another example, unpublished results of tuft studies on an autogiro rotor (for conditions generally similar to those reported on in reference 3) have shown both types of flow, with the outward flow occurring subsequent to the forward flow during each revolution. Thus we have anomalies in that the separated flow is sometimes two dimensional and other times three dimensional. For the first case the readily available two-dimensional airfoil characteristics can be expected to be usable; for the second case they cannot, and differences could be large as well as relatively unknown. Along the same lines, the pressure-distribution studies reported in references 4 and 5 show normal two-dimensional chordwise distribution-plot shapes (see the upper portion of figure 1) for normal-force coefficients $\left(C_N\text{'s}\right)$ up to and even above the maximum C_N obtained in static two-dimensional tests. Shapes such as that shown in the lower portion of figure 1 are found in references 4 and 5 for still higher C_N's, for example, 1.7. The $C_N = 1.7$ plot represents at least a local lift bonus over the static two-dimensional case but also represents a pitching-moment change which may tend to discourage operation at this condition in spite of any net increase in lift capability. Reference 6 shows that for a hovering, unstalled blade the boundary-layer flow is surprisingly two dimensional. Reference 7, on the other hand, shows evidence of lift-curve extension on a propeller. This problem of at least part-time deviations from the conveniently available

two-dimensional data, then, seems largely confined to angles of attack above the simple-flow-condition stall angle.

In the case of swept wings for airplanes, it has surely paid off to understand the difference in maximum-lift flow mechanism as contrasted to that for straight wings. Similarly, an understanding of the corresponding flow mechanism for the rotor blade should pay off. One possible experimental ingredient for better understanding - testing of several full-scale rotors - will be discussed in a later section.

Jet-Flap Rotor

Thus far, the question of what lift can be obtained from a conventional blade has been considered. It is interesting to note that at least one high-lift device has shown promise of working in a rotor. Specifically, reference 8 reports successful wind-tunnel operation of a full-scale rotor at a tip-speed ratio of 0.5 and a blade mean lift coefficient $\bar{c}_l$ (computed as $6C_T/\sigma$) of about one. Ordinarily, retreating-blade stall effects stop such tests far short of this combination.

Blade-Motion Problems in Maneuvers

Another aspect of maneuver capability is blade-motion stability, blade-motion amplitude, and blade-motion vibration content. Some recent analytical studies in this connection are reported in reference 9. In that paper, the importance of the "aerodynamic spring" action on the forward blade is explained. It is shown that, for nonlinear calculations, both the blade stability and transient response are drastically affected by the blade azimuth angle at which a gust (or control) disturbance is introduced. It is also shown

that blades lacking a high margin of stability will, as compared to highly damped blades, be more prone to go out of track and cause vibration when disturbed. These studies are illustrative of analytical treatments which can profitably be extended to include more and more degrees of freedom, provided that validity can be established for the successive ingredients. Such work should help increase the usable steady value of normal acceleration, which tends, at cruise speeds, to be limited by vibration and blade behavior.

Remote-Controlled Flight-Test Articles

As a final point concerning the maneuver problem, it seems necessary that both in extending the aerodynamic maximum, and in trying to close the large gap between practical operating limits and this aerodynamic maximum, flight tests must be a part of the picture. Because of the general violence and the nonlinearities and uncertainties involved, it is suggested that the use of pilotless, instrumented, remote-controlled drone helicopters is worth serious consideration for the most advanced testing, both for research and development purposes.

CRUISE EFFICIENCY

Progress is being made in reducing helicopter fuselage drag; also, blade tip speeds have risen. These factors make blade-section profile losses stand out as a major source of cruise power loss, and lead to reconsideration of laminar-flow airfoil sections.

Laminar-Flow Sections

Blade construction methods have improved immensely since the period when laminar-flow sections were previously tried and considered impractical. Current construction methods, with some care and adaptation, may provide sufficient contour accuracy for holding laminar flow. Extra processing for accuracy could, if desired, be restricted to the outer part of the blade since this high-speed portion accounts for most of the skin-friction type loss. Incidentally, difficulties in holding laminar flow on airplanes do not provide an adequate precedent because the typical airplane-wing Reynolds number is so much higher than that for a rotor blade.

Before re-embarking on laminar-flow-airfoil efforts, however, it appears extremely important to determine with actual rotor blades the effect on the ability to hold laminar flow of a variety of practical operating problems such as accumulation of bug spatters, salt, and dirt. Such checks should include the outer portion of current wide-chord, main-rotor blades, since high Reynolds number makes the problem harder. The photographic transition-check method described in reference 6 would seem well suited.

ALL-AROUND BLADE AERODYNAMIC REDESIGN

Since maneuver capability as well as cruise efficiency call for blade redesign, a broader look than just given seems in order. In fact, there are so many indications of intent to work toward "optimum" blade designs, with radial variations in thickness, camber, chord, and the like, and of intent to work with inboard-download problems as well as outboard

profile losses, that the question seems to be how, rather than whether, such effort should take place.

Tip Stall Versus Inboard Stall

First of all, perhaps a reminder is in order that when helicopter blades with no twist were used they really did stall first and most seriously in the region of the blade tips. Figure 2, taken from reference 1, illustrates this point.(1) On the other hand with -8° twist, even the elementary uniform-inflow theory typically predicts stall earlier at the 3/4 radius than at the tip, making it reasonable to entertain the use of thinner, less cambered, smaller nose-radius sections toward the tip.

Tolerable Camber

Second, since a blade with no camber anywhere will be far from optimum aerodynamically, it seems imperative that the issue of judicious use of camber. versus no camber anywhere, be resolved. At one extreme, hopes for the practical success of the jet flap as actually tested in reference 8 hinge on the ability to tolerate really large blade-section pitching moments. At the other extreme, there are still reports of serious vibration being caused by extremely small camber values, even with uniformly built sets of blades. Then again, the really obvious excesses of camber through history have involved amounts so large as to twist off part of a blade or to cause narrow

(1)Note: Because no tufts were mounted at the extreme tip and there is reason to believe the flow on the outer few percent may remain unseparated, the outer rim of the shading shown in the original plot of reference 1 is removed in figure 2.

escapes from outside loops. Carefully conducted measurements of effect of small camber increments on vibration for several established helicopter types would seem a prerequisite to selection of airfoils for improved rotors.

Systematic Series of Full-Scale Blades

Third, it is believed that the accurate construction and careful flight testing of at least a modest-sized series of full-scale blades with systematically varied parameters is now warranted. One reason for such a step is that anomalies such as those mentioned in the "maneuver capability" section cast too much doubt on theoretical and small-scale-model work. Such full-scale information should provide clues and anchor points which would greatly aid the other types of effort planned toward resolving these anomalies and doubts. Perhaps such rotors should be built in groups of two or three, with six months to a year between groups to allow as much of the progress as possible to be made by way of more varied, interrelated small-scale and theoretical work. In view of the large production rates for helicopters, the cost might be warranted as a part of developmental product-refinement work.

Conditions for Adequacy of Current Theory

Before leaving the subject of blade design it is desired to emphasize that the anomalies referred to relate to cases where sizable areas involve blade-section conditions inviting flow separation. When, instead, all regions other than those for very low dynamic pressures involve low or moderate angles of attack, analyses based on simple two-dimensional characteristics normally seem to be quite appropriate and adequate.

NOISE, BLADE STRESS, AND VIBRATION

These well-known problems have one aspect in common, and that is the only aspect to be discussed here. Specifically, the striking of tip vortices left by preceding blades is widely believed to contribute to all of these problems. Considerable proof to this effect has been accumulated and need not be reviewed here.

Since this vortex is such a nuisance, it seems reasonable to illustrate a few preliminary ideas on ways to shift it partly out of the way as well as to weaken it. Blowing a sheet of air from the end of a lifting surface (fig. 3, part 1) will, per reference 10, move the tip vortex outward. It is also shown in reference 10 that blowing at a downward angle can hold the vortex down and, to judge from the flow photographs, weaken or spread it - all beneficial for the purpose at hand. Blowing straight out in plan view is unthinkably wasteful; if done from a swept tip as in part 2 of figure 3, however, about 85 percent of the jet energy would be used to turn the rotor, and the scheme can be looked upon as a modification to a tip-jet-driven rotor. Tip sweep may have an additive effect in reducing noise, as well as serving to delay Mach number problems of all types, but introduces structural problems; also, the position of the jet sheet in relation to the solid surface may be important. Alternate arrangements are therefore illustrated in parts 3, 4, and 5 of figure 3. No data have been found to show whether or not the sweptback sheet of air would retain its vortex-control effectiveness. If it does, and if the increased effective blade radius (caused by shifting the vortex out) should roughly offset the 15 percent loss, then on a tip-driven

design reductions in noise and vibration should be obtainable essentially "for free."

As a more prosaic aspect of the tip-vortex noise and vibration problem, the effects on it of changes in planform and twist should be one of the factors assessed during the previously discussed systematic variation of blade aerodynamic design.

WAYS TO LEARN MORE FROM WHAT IS BEING DONE

In the midst of machine computations and quantities of data, the simpler ways of learning things, and of keeping track of where we stand, or of making quick comparisons with past experience, tend to get lost. The simpler techniques can still be of value, and a few examples follow.

Rules of Thumb and Indexes

In reference 11, a rule of thumb for estimating available all-out, demonstration-type-maneuver normal acceleration was suggested; that is, to take the ratio of section $c_{l_{max}}$ to level-flight blade mean lift coefficient $\bar{c}_l$. Where the needed data have been obtainable, this crude approach has come surprisingly close. One inference is that since there have to be some areas operating materially below $c_{l_{max}}$, some local c_l values above the simply determined $c_{l_{max}}$ are being realized.

The struggle with the blade-stall anomalies mentioned earlier makes it important to know how consistently this happens. As an even simpler approach than comparing with the complete rule of thumb, perhaps the initial reporting of well-instrumented severe maneuvers could include the value of

$\bar{c}_1$ corresponding to maximum acceleration. This step should be very easy for the original investigator, though it seems usually to become impossible for someone else at a later date. This number should, incidentally, be of great interest to those conducting such flights, as well as to the specialist in rotor aerodynamics.

A modern criterion for estimating practical operating limits (such as may be imposed by blade stall, for example) is given in reference 12, based in essence on rate of power rise with change in flight condition. The tables of reference 13 provide supplementary numbers which indicate, from power considerations, the severity of conditions between or beyond the criterion values. With experience this approach may give a good measure for conditions beyond the criteria boundaries, of severity from considerations of overall behavior including vibration and controllability. It would appear that a gap will remain, however, at least in respect to the margin of safety between a mild condition and a limiting condition; that is, where the power changes are relatively insensitive to operating conditions. To fill this gap, an index similar to the long-familiar simply estimated tip and inboard retreating-blade angles of attack is still needed. Perhaps the statement of $\bar{c}_1$, tip-speed ratio μ, and tip-path-plane angle of attack would help - or perhaps the simply estimated angles of attack just mentioned, if both were given together with μ, would still serve.

Comparison With One Theory

As more experimental results on rotor aerodynamics become available, their cross-comparison will be aided if each is compared to the same rotor theory. To aid this process, NASA has recently published references 12

and 13, providing easily used though basically complex theory. Comparison with this easily used theory need not be viewed as taking the place of any hand-tailored calculations deemed desirable, but rather as a supplementary step to aid data interpretation and, in conjunction with similar comparisons for other tests, to help judge needed additions to or modifications in current theory.

Pooling of Computer Processes

Studies such as that of reference 9 (blade dynamic behavior) can profit from extensions to include more and more coupled degrees of freedom in the rotor dynamics treatment. Numerous organizations have, and will continue to, set up computing-machine processes for adding coupled degrees of freedom. Perhaps a serious effort should be made to find ways to pool or otherwise make multiple use of established computing-machine programs or results, especially when these programs or results are either mechanical tools seperable from creative thinking, or else represent a clear-cut stage of refinement.

CONCLUDING REMARKS

Anomalies in rotor aerodynamics have been pointed out, the resolution of which would help solve recognized problems. It appears that the inevitable efforts to develop "optimum" rotor blades might be guided in such a way as to help resolve these anomalies. Several steps which should in any case precede major effort towards aerodynamically improved rotors have been suggested. Finally, as means toward more efficient progress, it has been suggested that the practice of using simple rules of thumb be continued, that

outwardly unrelated test results be cross-compared through one theory, and that serious thought be given to means for pooling complex computing-machine programs for basic ingredients in rotor aeroelastic treatments.

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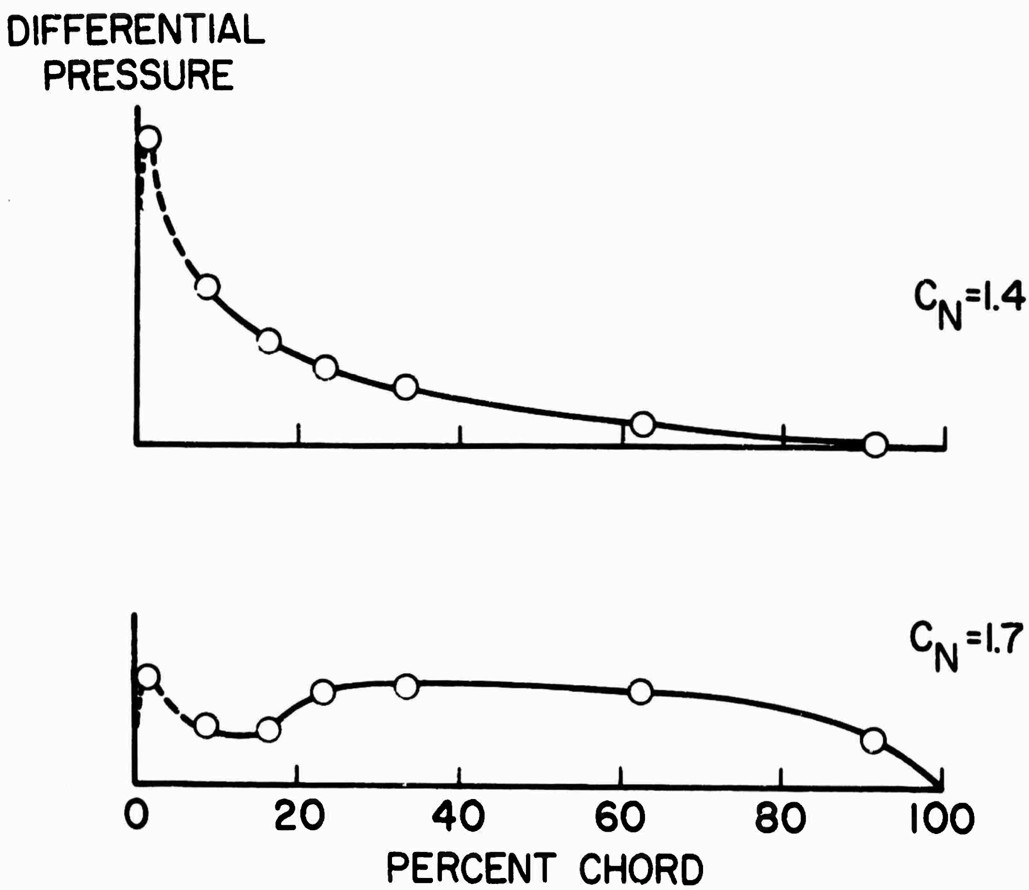

Figure 1.- Chordwise pressure distribution shapes.

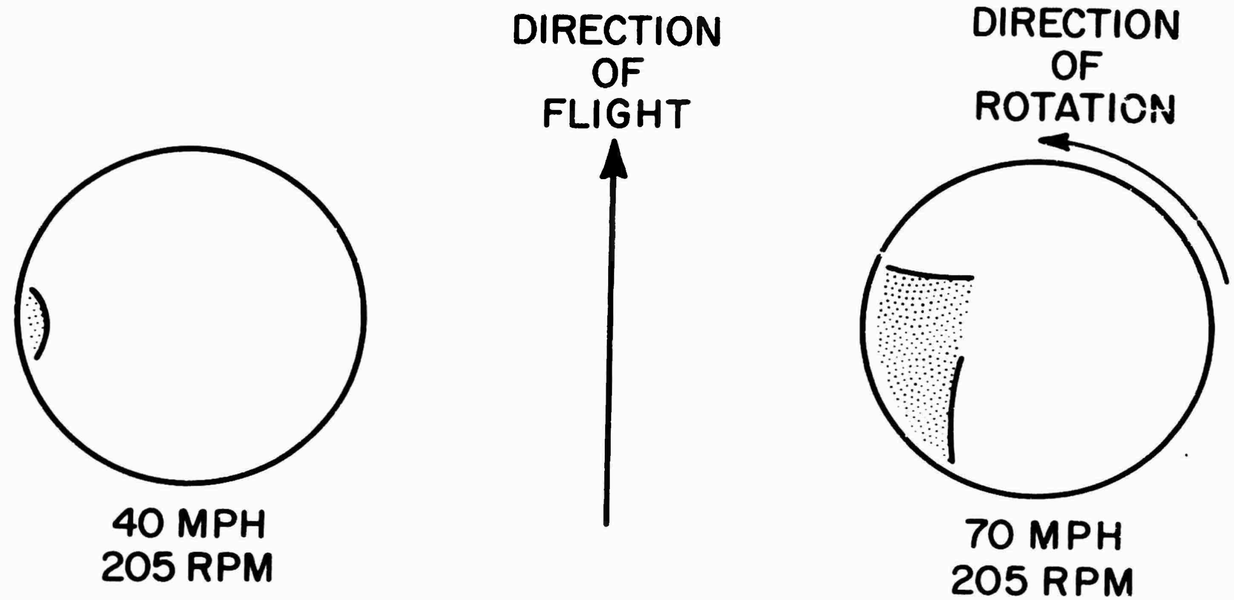

Figure 2.- Experimental retreating-blade stall areas for untwisted helicopter blades; plan view of rotor. (Based on the studies of NACA Rept. 840.)

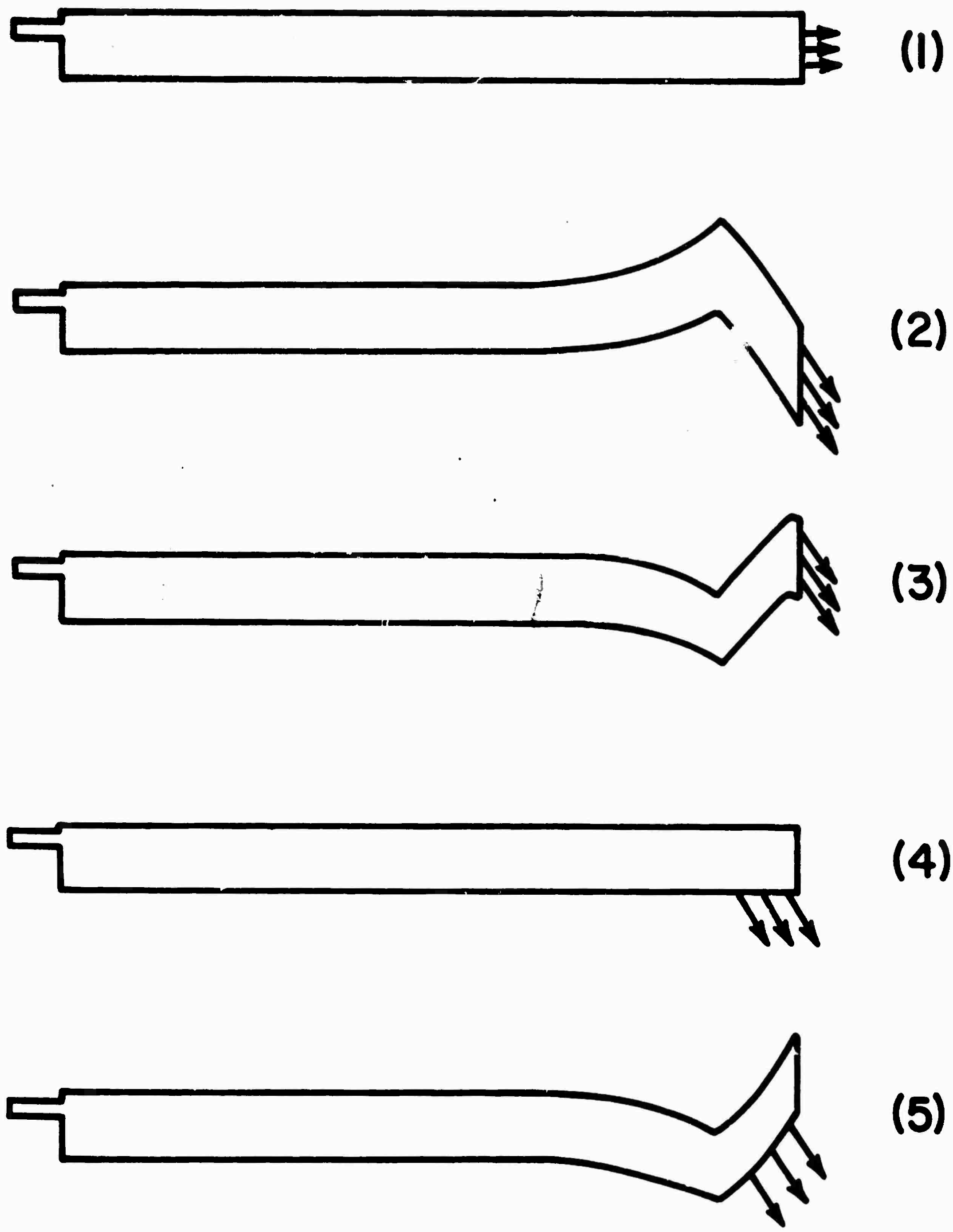

Figure 3.- Plan view of tip-blowing arrangements for altering blade-tip vortices.

SOME POSSIBILITIES FOR RESEARCH ON

STABILITY AND CONTROL

AT STOL FLIGHT SPEEDS

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SUMMARY

Consideration of the stability and control characteristics of the STOL aircraft suggests that useful research analyses can be developed from the classical linearized equations of motion, which use the concept of stability and control derivatives. For the STOL aircraft, the flight path gradient and the lift coefficient may be unusually large, and there may also be a requirement to retain those derivatives which describe aerodynamic lag effects. In consequence, the STOL theory contains more than the usual number of parameters. Some simplification can be achieved for the lateral motion by the definition of modified derivatives. The STOL aircraft can then be described in terms of an equivalent vehicle in level flight without inertial cross coupling between roll and yaw. The resultant equations are similar to those for conventional aircraft, and so there is a fund of known results which may be interpreted and applied in analyses for STOL aircraft.

The linearization of the equations of motion for the STOL aircraft

leads quite naturally to a consideration of transfer functions and impulsive admittances. Useful theoretical ideas may be stimulated by the admittances because they represent time histories of the motion which can be considered as a sum of contributions arising from the controls fixed modes of motion. In general, the various modes have quite distinctive characteristics and, in consequence, it is expected that new ideas and analysis techniques will be stimulated by intuitive appraisals of the motion variables.

The handling qualities research for the STOL aircraft must often consider the influences of atmospheric turbulence and wind shear. For example, the roll power requirements for small aircraft may be greater than those for large aircraft, and this could possibly be related to the effects of atmospheric turbulence. Realistic theoretical analyses for the flight of STOL aircraft appear to be beyond the capability of the available data and theories. However, it is suggested that certain simplified analyses may prove to be effective for flight in turbulent air.

INTRODUCTION

The STOL aircraft performs both the take-off and landing at high values of the lift coefficient and along flight paths with steep gradients. The landing strips are typically small and are often located close to obstructions which require precise manoeuvres, and also give rise to turbulence along

the flight path. Quite clearly the requirements of stability and control for the STOL aircraft require specialized study.

A comprehensive rational analysis of the stability and control requirements of the STOL aircraft is beyond the capability of the available theoretical techniques and it seems that considerable reliance will have to be placed on simulation studies and operational experience.

Linearized theoretical analysis of the motion of the STOL aircraft may also prove to be of value, and some possible research and analysis techniques are discussed herein.

THEORETICAL CONCEPT

In this report, our remarks are based on the premise that the motion of the STOL aircraft can be adequately described by the linearized equations of motion. It is then a simple analytical matter to include in these equations, the approximate effects of aerodynamic lag, and the steep flight path gradients characteristic of the STOL aircraft. The equations are then susceptible to solution by classical analytical techniques which make use of an automatic digital computer for specific numerical studies. The above procedures will be applicable provided that the aircraft motions are not too violent, and we may usually consider the longitudinal and lateral motions separately.

For example, we will illustrate our remarks with the lateral equations of motion and concentrate our attention on the five equations that describe the side force, the rolling and yawing moments, and the kinematical relationships for the motion. The above five relationships give five equations, and following the method of Etkin (Reference 1), we may express these relationships in the Laplace transform notation as follows:

$$\left((2\mu - C_{Y_{\dot\beta}})s - C_{Y_\beta}\right)\bar\beta - C_{Y_p}\bar p + (2\mu - C_{Y_r})\bar r - C_{L_0}\bar\phi = -\sum\left(C_{Y_{\dot\delta}}s + C_{Y_\delta}\right)\bar\delta$$

$$-\left(C_{l_{\dot\beta}}s + C_{l_\beta}\right)\bar\beta + (i_A s - C_{l_p})\bar p - (i_E s + C_{l_r})\bar r = -\sum\left(C_{l_{\dot\delta}}s + C_{l_\delta}\right)\bar\delta$$

$$-\left(C_{n_{\dot\beta}}s + C_{n_\beta}\right)\bar\beta - (i_E s + C_{n_p})\bar p + (i_C s - C_{n_r})\bar r = -\sum\left(C_{n_{\dot\delta}}s + C_{n_\delta}\right)\bar\delta$$

$$\bar p + (\tan\theta)\,\bar r - s\bar\phi = 0$$

$$(\sec\theta)\,\bar r - s\bar\psi = 0$$

The above equations retain the terms required for studies of STOL aircraft. The concept of aerodynamic lag is included, approximately, by the inclusion of the "dot" derivatives for sideslip and control effects. The other "dot" derivatives would give apparent mass and inertia effects which could be included in the inertia terms if desired.

On the right hand side of the equation, we have written a summation

to represent the control inputs from aileron, rudder, differential thrust, etc.

Except for the inclusion of some additional derivatives, the above equations are the same as for conventional aircraft. Hence, we have available a well developed fund of knowledge to assist in our studies. On the other hand, we will expect to encounter unusual numerical values of many of the quantities and in consequence we will expect to find a number of effects which are new and of special interest.

It is characteristic of the STOL aircraft that the lift coefficient is unusually high and, in addition, the flight path gradient and product of inertia effects may also be large. To avoid complications in the analyses, and to put the equations into a more convenient form, it is desirable that the flight path gradient and product of inertia be eliminated as explicit parameters. This result is possible simply by a redefinition of the derivatives so that they implicitly include these parameters.

A suitable form of the equations can be obtained by eliminating $\bar{\hat{p}}$ and $\bar{\hat{r}}$ in the side force, and rolling and yawing moment equations. We also avoid a dependence of the rolling moment on $s^2 \bar{\psi}$, and of the yawing moment on $s^2 \bar{\phi}$, by eliminating these two terms between the rolling and yawing moment equations. The following equations are then obtained:

$$\left(s-\hat{C}_{Y_\beta}\right)\bar{\beta} - \left(\hat{C}_{Y_p} s+\hat{C}_{L_0}\right)\bar{\phi} + \left(1-\hat{C}_{Y_r}\right) s\,\bar{\psi} = -\sum\left(\hat{C}_{Y_{\dot{\delta}}} s+\hat{C}_{Y_\delta}\right)\bar{\delta}$$

$$-(\hat{C}_{\ell_{\dot{\beta}}} s + \hat{C}_{\ell_\beta})\bar{\beta} + (s - \hat{C}_{\ell_p}) s \bar{\phi} - \hat{C}_{\ell_r} s \quad \bar{\psi} = -\sum (\hat{C}_{\ell_{\dot{\delta}}} s + \hat{C}_{\ell_\delta}) \bar{\delta}$$

$$-(\hat{C}_{n_{\dot{\beta}}} s + \hat{C}_{n_\beta})\bar{\beta} - (\hat{C}_{n_p}) s \bar{\phi} + (s - \hat{C}_{n_r}) s \bar{\psi} = -\sum (\hat{C}_{n_{\dot{\delta}}} s + \hat{C}_{n_\delta}) \bar{\delta}$$

$$\bar{\hat{p}} + (\tan \theta)\bar{\hat{r}} - s\bar{\phi} = 0$$

$$(\sec \theta)\bar{\hat{r}} - s\bar{\psi} = 0$$

The first three of the above equations are linear, algebraic expressions in the transformed variables $\bar{\beta}$, $\bar{\phi}$, and $\bar{\psi}$ only, and so, solutions may be obtained without reference to the final two equations. Because we need not consider explicitly the effect of flight path gradient or product of inertia, we may imagine that we are dealing with an equivalent dynamic system in level flight and without roll-yaw inertial cross coupling. Such a simple form of the equations has commonly been applied in the analyses of conventional aircraft. However, for the STOL aircraft it may be necessary to consider the effects of aerodynamic lag as described in the above equations by the "dot" derivatives.

The above concept of the modified derivatives permits a reduction in the number of parameters, which should be an advantage for the analysis and interpretation of data.

POSSIBLE RESEARCH TECHNIQUES FOR THE ANALYSIS OF THE STABILITY AND CONTROL CHARACTERISTICS OF STOL AIRCRAFT

The Transfer Function

In many studies of stability and control, the transfer function has been employed to describe the vehicle response to an actuation of the controls. Suppose for example, that we wish to know the aircraft response in bank angle, ϕ, to a deflection of one of the controls, δ_c. We can solve the equations given in the preceeding section for $\bar{\phi}$ in terms of $\bar{\delta}_c$, and we obtain a solution in the form:

$$\bar{\phi} = \frac{N(s)}{D(s)} \bar{\delta}_c$$

where $\frac{N(s)}{D(s)}$ is the transfer function and $\bar{\phi}$ and $\bar{\delta}_c$ are the Laplace transforms of the bank and control angles. The transfer function is clearly a fundamental parameter for describing the dynamic system.

In many research studies, data have been correlated with various coefficients of the transfer functions. However, difficulty has been encountered in selecting a single coefficient to describe the variations in the system, and it is then difficult to obtain concise correlations. Furthermore, from the engineering point of view, it may not always be possible to simply appreciate the significance of the various coefficients of the transfer function, and so there is an incentive to seek alternative analytical techniques. We, therefore, turn our attention to the impulsive admittance.

The Impulsive Admittance

The impulsive admittance like the transfer function is a fundamental

characteristic of the dynamic system. But unlike the transfer function, it represents a time history of a motion variable. Mathematically the impulsive admittance is the inverse Laplace transform of the transfer function and obviously it must contain the same information but in a different form.

It is interesting to note that the impulsive admittance to a lateral control input, consists of a sum of the three fundamental, controls fixed modes of motion for both the STOL and conventional aircraft. Hence, the motion is composed of the roll, spiral, and dutch roll modes.

The response to any arbitrary time history of the control motion can be deduced from the impulsive admittance by an application of the Duhamel's (convolution) integral (Reference 1).

The potential usefulness of the impulsive admittance stems from the fact that the motion is described in terms of the familiar, three fundamental modes of motion, each of which has quite distinct characteristics. In consequence, engineering intuition may be found to be effective for stimulating new ideas and analysis techniques.

For example, let us consider the response in ϕ to an impulsive application of the aileron. If we assume that the aileron has no aerodynamic lag, then the impulsive admittance may be written as:

$$\phi(t) = A_R e^{\tau_R \hat{t}} + A_S e^{\tau_s \hat{t}} + A_{DR} e^{n\hat{t}} \sin(\omega\hat{t} + \epsilon)$$

where the initial amplitudes of the roll, spiral, and dutch roll modes are

A_R, A_S, and A_{DR} respectively.

It is perhaps disappointing that the impulsive admittance for lateral controls contains so many parameters even for the simplified case where there is no lag in the controls. However, we will attempt to show that each of the eight parameters of the impulsive admittance can be assessed in terms of its significance for the motion of the aircraft. In fact, we will find that intuitive arguments can be developed which allow the development of important ideas.

Let us, for example, consider the aileron control and how it is used to control the aircraft. Suppose the aircraft is flying straight and level and the pilot wishes to change the flight path and execute a steady turn. We assume that he will wish to apply aileron for a short time and then return the aileron to a more or less neutral position after the steady turn is established. In the abstraction of mathematics, we may represent the above practical situation by assuming an impulsive actuation of the ailerons.

The equation given above for the impulsive admittance shows that each of the three fundamental, controls-fixed modes of motion is excited by the ailerons and the motion is as illustrated in Figure 1. It is preferable that the spiral mode is almost neutrally stable, in which case, the aircraft proceeds from one steady bank angle to another which is nearly steady. If the aircraft proceeds smoothly and quickly from one steady bank angle to another, then the pilot can easily anticipate the aircraft motion, and in

consequence the associated piloting tasks are simple. We see that a change in excitation of the spiral mode is the primary desired result of an application of the aileron control.

The rolling mode forms another component of the motion (Figure 1), and this mode is preferably a convergence with a short time constant. We see from the time history that the rolling mode makes possible a smooth transition between different values of the spiral mode. If the time constant of the rolling mode is too short, then the motion will be too abrupt or jerky and the pilot will complain of excessive rolling accelerations. On the other hand, if the time constant is too long, then it will be difficult for the pilot to anticipate the aircraft response. Accordingly, the time constant of the rolling mode should lie within a restricted range of values. We can see also that the amplitude of the roll convergence mode is automatically that which gives a smooth motion and so the amplitude of the roll mode need not be considered explicitly.

Finally, we note that the impulsive admittance contains a dutch roll excitation (Figure 1). We suggest that the dutch roll excitation performs no useful function and the initial amplitude of the dutch roll may be treated as a measure of a deficiency of the aileron control. This idea has been used in some simulator studies except that the amplitude of the first overswing was used instead of the initial dutch roll amplitude (Reference 2).

It is suggested that the above discussions show that the impulsive

admittance concept leads to a useful technique for the study of aileron controls.

Analysis of the functions of the rudder control would appear to be more complex, even with the use of the impulsive admittance concept. Studies of longitudinal control employing the above techniques would also be expected to prove useful. Hence, it is suggested that the impulsive admittance techniques naturally lead to further research studies.

Calculation of the Impulsive Admittances

The impulsive admittances for the lateral and longitudinal motions can be computed conveniently with an automatic digital computer, and there are no difficulties associated with the retention of the derivatives which are of interest for the STOL aircraft. The calculations can then be employed in various studies of the aircraft motion.

As an illustration of a particular problem, let us assume that there are no control lag terms, and let us suppose that we wish to operate the rudder in such a way that there will be an improvement in the aircraft response to aileron control. We start by specifying that the dutch roll component of the aircraft response should be reduced or eliminated. There should also be an adequate response in the bank angle ϕ for a given aileron input.

Let us now examine what happens when the rudder and aileron are applied simultaneously. We find that there will be a certain ratio of rudder

to aileron angle that will result in a minimum dutch roll excitation. However, this minimum may be large enough to be objectionable. From a study of the impulsive admittances, we see that this situation arises because the phase angles of the dutch roll excitation for aileron and rudder are different and so it is impossible to achieve a complete cancellation of the dutch roll. However, if we introduce a suitable phase angle between the aileron and rudder inputs, it is possible to cancel out the dutch roll entirely at the time of the second control input. Hence, the dutch roll only exists during the time interval between the aileron and rudder inputs. As an example of this phase angle technique, we show an aileron application followed by a suitably phased rudder input, (Figure 2). We see that the bank angle response is adequate and that there is a smooth and rapid increase of the bank angle to the value corresponding to the spiral mode.

Similar analyses could be applied if differential thrust were used as a control, either in conjunction with the aileron and rudder, or all three simultaneously.

It is encouraging to note that the concept of the impulsive admittance may prove to be a useful research tool. The above examples suggest that it is capable of providing a simple, clear insight into some of the stability and control problems of the STOL aircraft. These examples represent only a beginning and it is suggested that many valuable results could be obtained from further studies.

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C_{y_0}, C_{ℓ_0}, and C_{n_0} are the aircraft aerodynamic asymmetries.

The steady state control equations form a set of three linear algebraic equations in four unknowns. Solutions for any three variables can be obtained in terms of the fourth. It is perhaps instructive to solve for ϕ, C_n and C_ℓ in terms of β. We then obtain solutions of the form:

$$\phi = \phi_0 + \frac{1}{C_L} k_\phi \beta$$

$$C_\ell = C_{\ell_0} + k_{C_\ell} \beta$$

$$C_n = C_{n_0} + k_{C_n} \beta$$

For the aircraft to have conventional control characteristics. ϕ_0, C_{ℓ_0}, and C_{n_0} must be acceptably small. In addition, the coefficients $\frac{1}{C_L} k_\phi$, k_{C_ℓ}, and k_{C_n} must lie within a certain range of values, taking into account the sign of the coefficients.

The need to achieve a certain range of values of $\frac{1}{C_L} k_\phi$ is perhaps not entirely clear at this time. Some preliminary research results suggest, however, that very small values of $\frac{1}{C_L} k_\phi$ may lead to problems of control. This result is interesting because we see at once that the high values of C_L associated with STOL contributes to the problem. Furthermore, it is possible to encounter the situation where k_ϕ also becomes small at high C_L and it could in fact change sign.

Further study in this area could perhaps cast some light on the significance of the $\frac{1}{C_L} k_\phi$ parameter.

For steady turning flight, we find that the STOL aircraft may encounter some special problems, because the radius of turn may be approaching the order of magnitude of the wing span. Even in the linearized equations for steady turning flight, we encounter situations where the conventional equations are no longer valid. For example, if we consider a STOL aircraft in steady turning flight along a helical flight path, (Figure 3) we observe that a change in the aircraft sideslip angle, β , results in a change in the aircraft orientation, defined according to the method of Figure 4. In consequence, forces and moments are introduced which have not been retained in conventional analyses. The lateral equations of motion for the STOL aircraft may be written as:

$$C_{Y_\beta}\beta + C_{Y_p}\hat{p} + C_{Y_r}\hat{r} + C_{Y_{\delta A}}\cdot\delta A + C_{Y_{\delta R}}\cdot\delta R = C_{Y_o} + C_W n_Y$$

$$C_{\ell_\beta}\beta + C_{\ell_p}\hat{p} + C_{\ell_r}\hat{r} + C_{\ell_{\delta A}}\cdot\delta A + C_{\ell_{\delta R}}\cdot\delta R = C_{\ell_o} + (i_C - i_B)\hat{q}\hat{r} - i_E\hat{p}\hat{q} + h_Z\hat{q} - h_Y\hat{r}$$

$$C_{n_\beta}\beta + C_{n_p}\hat{p} + C_{n_r}\hat{r} + C_{n_{\delta A}}\cdot\delta A + C_{n_{\delta R}}\cdot\delta R = C_{n_o} + (i_B - i_A)\hat{p}\hat{q} + i_E\hat{q}\hat{r} + h_Y\hat{p} - h_X\hat{q}$$

The above equations are valid for symmetric mass distributions ($i_D = i_F = 0$).

If we note that in steady turning flight, change in β produces a

change in the aircraft orientation, we can deduce equations of the form:

$$\begin{bmatrix} C'_{Y\beta} & C'_{Y\Delta\phi} & C_{Y\delta A} & C_{Y\delta R} \\ C'_{\ell\beta} & C'_{\ell\Delta\phi} & C_{\ell\delta A} & C_{\ell\delta R} \\ C'_{n\beta} & C'_{n\Delta\phi} & C_{n\delta A} & C_{n\delta R} \end{bmatrix} \begin{bmatrix} \beta \\ \Delta\phi \\ \delta A \\ \delta R \end{bmatrix} = \begin{bmatrix} C'_{Y_0} \\ C'_{\ell_0} \\ C'_{n_0} \end{bmatrix}$$

where

$$C'_{Y\beta} = C_{Y\beta} - \left(C_{Y_p} \operatorname{Cos}\Theta_0 \operatorname{Sin}\phi_0 + C_{Y_r} \operatorname{Sin}\Theta_0 \operatorname{Sin}\phi_0 \operatorname{Cos}\phi_0\right) D\psi$$

$$C'_{\ell\beta} = C_{\ell\beta} + \left(-C_{\ell_p} \operatorname{Cos}\Theta_0 \operatorname{Sin}\phi_0 + h_Z \operatorname{Sin}\Theta_0 \operatorname{Sin}^2\phi_0 - [C_{\ell_r} + h_Y] \operatorname{Sin}\Theta_0 \operatorname{Sin}\phi_0 \operatorname{Cos}\phi_0\right) D\psi$$
$$+ \left([i_C - i_B]\, 2 \operatorname{Sin}^2\phi_0 \operatorname{Cos}\phi_0 \operatorname{Sin}\Theta_0 \operatorname{Cos}\Theta_0 + i_E \operatorname{Sin}^2\phi_0 \left[\operatorname{Sin}^2\Theta_0 - \operatorname{Cos}^2\Theta_0\right]\right)(D\psi)^2$$

$$C'_{n\beta} = C_{n\beta} + \left(-[C_{n_p} - h_Y] \operatorname{Cos}\Theta_0 \operatorname{Sin}\phi_0 - h_X \operatorname{Sin}\Theta_0 \operatorname{Sin}^2\phi_0 - C_{n_r} \operatorname{Sin}\Theta_0 \operatorname{Sin}\phi_0 \operatorname{Cos}\phi_0\right) D\psi$$
$$+ \left(-[i_B - i_A] \operatorname{Sin}^2\phi_0 \left[\operatorname{Sin}^2\Theta_0 - \operatorname{Cos}^2\Theta_0\right] + i_E\, 2 \operatorname{Sin}^2\phi_0 \operatorname{Cos}\phi_0 \operatorname{Sin}\Theta_0 \operatorname{Cos}\Theta_0\right)(D\psi)^2$$

$$C'_{Y\Delta\phi} = C_L - C_{Y_r} \operatorname{Cos} \quad , \operatorname{Sin}\phi_0\, D\psi$$

$$C'_{\ell\Delta\phi} = \left(-h_Z \operatorname{Cos}\Theta_0 \operatorname{Cos}\phi_0 - [C_{\ell_r} + h_Y] \operatorname{Cos}\Theta_0 \operatorname{Sin}\phi_0\right) D\psi$$
$$+ \left([i_C - i_B] \operatorname{Cos}^2\Theta_0 \left[\operatorname{Sin}^2\phi_0 - \operatorname{Cos}^2\phi_0\right] - i_E \operatorname{Sin}\Theta_0 \operatorname{Cos}\Theta_0 \operatorname{Cos}\phi_0\right)(D\psi)^2$$

$$C'_{n\Delta\phi} = \left(h_X \operatorname{Cos}\Theta_0 \operatorname{Cos}\phi_0 - C_{n_r} \operatorname{Cos}\Theta_0 \operatorname{Sin}\phi_0\right) D\psi$$
$$+ \left([i_B - i_A] \operatorname{Sin}\Theta_0 \operatorname{Cos}\Theta_0 \operatorname{Cos}\phi_0 + i_E \operatorname{Cos}^2\Theta_0 \left[\operatorname{Sin}^2\phi_0 - \operatorname{Cos}^2\phi_0\right]\right)(D\psi)^2$$

$$C'_{Y_0} = C_{Y_0} + \left(C_{Y_p} \sin\Theta_0 - C_{Y_r} \cos\phi_0 \cos\Theta_0\right) D\psi$$

$$C'_{\ell_0} = C_{\ell_0} + \left(C_{\ell_p} \sin\Theta_0 + h_Z \sin\phi_0 \cos\Theta_0 - [C_{\ell_r} + h_Y] \cos\phi_0 \cos\Theta_0\right) D\psi$$
$$+ \left([i_C - i_B] \sin\phi_0 \cos^2\Theta_0 \cos\phi_0 + i_E \sin\Theta_0 \cos\Theta_0 \sin\phi_0\right)(D\psi)^2$$

$$C'_{n_0} = C_{n_0} + \left([C_{n_p} - h_Y] \sin\Theta_0 - h_X \sin\phi_0 \cos\Theta_0 - C_{n_r} \cos\phi_0 \cos\Theta_0\right) D\psi$$
$$+ \left(-[i_B - i_A] \sin\Theta_0 \cos\Theta_0 \sin\phi_0 + i_E \sin\phi_0 \cos^2\Theta_0 \cos\phi_0\right)(D\psi)^2$$

The above equations have retained the gyroscopic terms arising from the rotations of the aircraft and the rotating masses of the engines and propeller, etc. Whether these terms are of any particular quantitative significance for the STOL aircraft has yet to be determined.

The above steady turning flight analyses may be of interest for the study of control requirements in the approach to the stall, where the aircraft operates at very high lift coefficients with a resultant small radius of turning. Of course, the approach to the stall is typically a dynamic manoeuvre but it may be of interest to study the case where the lift coefficient is increasing very slowly. It may be desirable to determine whether this manoeuvre has an appreciable effect on the lateral control requirements.

The Measurement of Stability and Control Derivatives In Flight

There are many reasons for an interest in the measurement of the stability and control derivatives in flight. For example, wind tunnel measurements are influenced by the fact that it is difficult to carry out the

experiments without introducing excessive interference effects from the strut supports. Furthermore, the STOL wind tunnel tests tend to be restricted to low Reynolds numbers.

It is interesting to note that De Havilland has just completed a development and testing program designed to improve the techniques associated with the analysis of flight test data. The well known equations of motion transient response method was used for this program. In this method the equations of motion are written in terms of the variables β, $\hat{p}$, $\hat{r}$, ϕ, and δ corresponding to equations 1, 2, and 3 of this report. These variables and the required derivatives of these variables are measured at a number of observation times. If the mass and inertias are known, we then obtain for each time instant, a linear algebraic equation in which the unknowns are the stability and control derivatives. If a sufficient number of observations are made (say 100 or so), then solutions can be obtained by the method of least squares.

The innovation in the method of analysis proposed and tested by De Havilland involves the assumption that each of the measured quantities is subject to a constant error. These errors are grouped to form one constant which is an additional unknown in the equation. No specific use is made of this constant term itself. However, its inclusion in the analysis appears to significantly improve the precision of the solutions for the derivatives. The alternative of determining the constant errors from an

initial period of steady flight is less convenient, and implies that the values of the constant errors are based on information from this steady period only, instead of from the complete manoeuvre.

As an aid in the interpretation of the data, a "confidence interval" is attached to each of the measured stability and control derivatives. Typical results are shown in Figures 5 - 10.

The flight test program with the DHC-3 Otter aircraft has confirmed that accurate measurements can be made with commercially available instrumentation, and aerodynamic lag effects can in practice, be detected, (Figures 5 and 6).

The encouraging results from the flight experiments suggest that a special effort should be made to obtain information on the aerodynamic derivatives for flight at STOL speeds. Control lags may also be of interest because the reduced frequencies, $\frac{\omega c}{2u_0}$ for the control motions may be significant so that aerodynamic lag could be detected. In many cases, these reduced frequencies will be low enough for the lag effect to be described simply in terms of the "dot" derivatives. In any case, further research appears to be desirable.

Flight In Turbulent Air

For the STOL aircraft to be used effectively, it is essential that the pilot be able to execute the landing and take-off manoeuvres with precision, in spite of disturbances from atmospheric turbulence and wind shear. In

fact, STOL aircraft must be able to cope with especially severe turbulence in the sense that the turbulent velocity fluctuations will tend to be large relative to the flight velocity.

It is evident that the type of turbulence encountered by the STOL aircraft may be quite different from that encountered by the conventional aircraft and research studies would appear to be warranted in this area.

The response of the STOL aircraft to the type of turbulence encountered in close proximity to the ground may be difficult to compute for a number of reasons. First, the response of an aircraft to turbulence is computed in a statistical form, and the properties of the turbulence close to the ground may be significantly different from the more familiar turbulence at higher altitudes. Secondly, there may be some difficulty in describing the forces and moments acting on the aircraft if the aircraft size approaches the significant wavelengths of the turbulence. A need for further research in this respect appears to be evident.

From another point of view, however, it may be possible to examine some problems associated with flight in turbulent air by adopting certain simplifying assumptions.

For example, consider the problems of control that may arise when the STOL aircraft requires no bank angle change to generate a sideslip angle in straight flight. For this problem, it may be useful to assume that the aircraft is flying at an altitude where the spectrum of turbulence is

known. We might also suppose that the aircraft is small compared to the turbulent wavelengths. Under such conditions, we could determine the aircraft response to mild turbulence by employing the techniques of the power spectra and frequency response characteristics.

We then say:

$$\phi(\Omega) = \left| K.M(\Omega) \right|^2 \phi_T(\Omega)$$

where $\phi(\Omega)$ is the power spectrum of the aircraft response.

$\left| K.M(\Omega) \right|$ is the modulus of the aircraft frequency response.

$\phi_T(\Omega)$ is the power spectrum of the atmospheric turbulence.

Ω is the wave number of the turbulence.

The above calculations can readily be carried out with an automatic digital computer and it is possible to define the data for a sufficient number of frequencies so that the shape of the $\phi(\Omega)$ power spectrum is adequately defined. In many cases, the response will be mainly at the dutch roll frequency and the data needs to be well defined at this point.

We note that the above calculations are only valid for a stable aircraft. We must, therefore, suppose that there is some device for automatically stabilizing the aircraft. This could be either a pilot or auto-pilot.

Suppose that the pilot or auto-pilot can be represented by controls

that are deflected in proportion to some function of the motion variables. This will change the aircraft frequency response characteristics. In general, we will expect to find that there is a large change in the aircraft response at low frequencies, which certainly agrees with our ideas as to the effect of the pilot operating the controls.

Suppose that we have the special type of aircraft which requires no bank angle to execute a straight steady sideslip. We expect to find that there are no problems of control for flight in turbulent air provided that the controls are moved in response to the appropriate variables such as bank angle, etc. For the auto-pilot, this poses no problems, however, for the human pilot it may be difficult to control the sideslip angle because the technique of keeping the wings level is not effective. Research in this area is suggested.

Another research area relates to the effect of aircraft size on the required roll control power. Specifically, we question whether a study of flight in turbulent air might reveal the reasons why a higher roll power seems to be required by the small aircraft. As a starting point, we might take the roll power requirements proposed by Bisgood (Reference 3), and attempt to deduce the requirements appropriate to larger and smaller aircraft. A rather arbitrary empirical approach to the problem suggests that aircraft might require a roll power in inverse proportion to the square root of the aircraft span as illustrated in Figure 11. Based on some published data, the above hypothesis is at least plausible. Perhaps a detailed study of

flight in turbulent air could result in an improved specification of the roll power requirements.

CONCLUSIONS

A number of suggestions have been made regarding possible areas of research, and certain techniques have been discussed with a particular emphasis placed on analysis. It is not intended that experiment be considered as a secondary research tool but rather that analysis may suggest better techniques for recording and transmitting the experimental data. In this view, some suggestions have been made for experiments to measure stability and control derivatives in flight. Measurements have also been suggested relating to the flight of the STOL aircraft in turbulent air of the type encountered by the STOL aircraft in the take-off and landing manoeuvre.

ACKNOWLEDGEMENT

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NOTATIONS

Angles are in radians.

Any consistent system of units for the dimensional quantities may be used.

C_L	lift coefficient	$-Z_a/\frac{1}{2}\rho U^2 S$
C_ℓ	aerodynamic rolling moment coefficient	$L/\rho U^2 S\ell$
C_{ℓ_o}	constant component of C_ℓ due to asymmetry	
C_{ℓ_p}	$\partial C_\ell/\partial\hat{p}$	
C_{ℓ_r}	$\partial C_\ell/\partial\hat{r}$	
C_{ℓ_β}	$\partial C_\ell/\partial\beta$	

$C_{\ell_{\delta A}}$ $\partial C_\ell / \partial \delta A$

$C_{\ell_{\delta R}}$ $\partial C_\ell / \partial \delta R$

C_n aerodynamic yawing moment coefficient $N/\rho U^2 S \ell$

C_{n_o} constant component of C_n due to asymmetry

C_{n_p} $\partial C_n / \partial \hat{p}$

C_{n_r} $\partial C_n / \partial \hat{r}$

C_{n_β} $\partial C_n / \partial \beta$

$C_{n_{\delta A}}$ $\partial C_n / \partial \delta A$

$C_{n_{\delta R}}$ $\partial C_n / \partial \delta R$

C_W coefficient of weight $W/ \frac{1}{2} \rho U^2 S$

C_Y aerodynamic side force coefficient $Y/\rho U^2 S \ell$

C_{Y_o} constant component of C_Y due to asymmetry

C_{Y_p} $\partial C_Y / \partial \hat{p}$

C_{Y_r} $\partial C_Y / \partial \hat{r}$

C_{Y_β} $\partial C_Y / \partial \beta$

$C_{Y_{\delta A}}$ $\partial C_Y / \partial \delta A$

$C_{Y_{\delta R}}$ $\partial C_Y / \partial \delta R$

C_Z $Z/ \frac{1}{2} \rho U^2 S$

D differential operator with respect to non-dimensional time $\frac{d}{d\hat{t}}$

g acceleration due to gravity

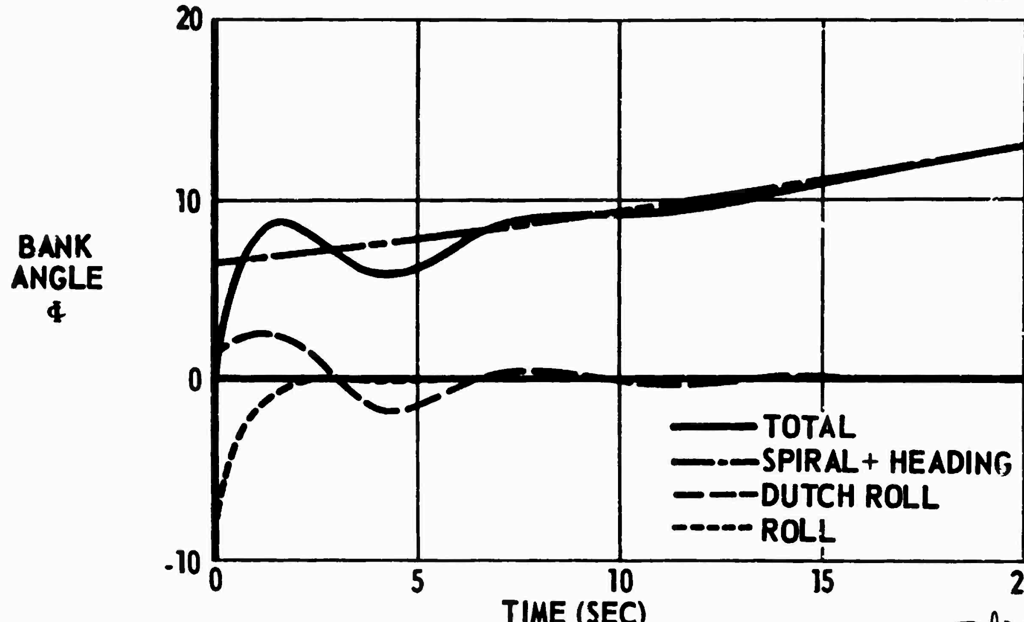
I_A aircraft moment of inertia about rolling axis OX

I_B aircraft moment of inertia about pitching axis OY

I_C	aircraft moment of inertia about yawing axis OZ
I_D	aircraft product of inertia about rolling axis OX and pitching axis OY
I_E	aircraft product of inertia about rolling axis OX and yawing axis OZ
I_F	aircraft product of inertia about pitching axis OY and yawing axis OZ
i_A	$I_A/\rho S \ell^3$
i_B	$I_B/\rho S \ell^3$
i_C	$I_C/\rho S \ell^3$
i_D	$I_D/\rho S \ell^3$
i_E	$I_E/\rho S \ell^3$
i_F	$I_F/\rho S \ell^3$
L	rolling moment
ℓ	reference length
N	yawing moment
n_Y	Y_B/W
p	rate of roll
$\hat{p}$	pt^*
q	rate of pitch
$\hat{q}$	qt^*
r	rate of yaw
$\hat{r}$	rt^*
S	reference area

t	time
t^*	ℓ/U
$\hat{t}$	non-dimensional aerodynamic time $\frac{t}{t^*}$
U	aircraft velocity (vector sum of all components)
W	aircraft weight
Y	aerodynamic side force
Y_B	body side force
Z	normal force or force along OZ stability axis
Z_O	normal force in truly banked zero sideslip conditions
β	sideslip angle
δ_A	aileron deflection
δ_R	rudder deflection
θ	flight path gradient relative to the horizontal plane
ρ	ambient air density
ϕ	bank angle

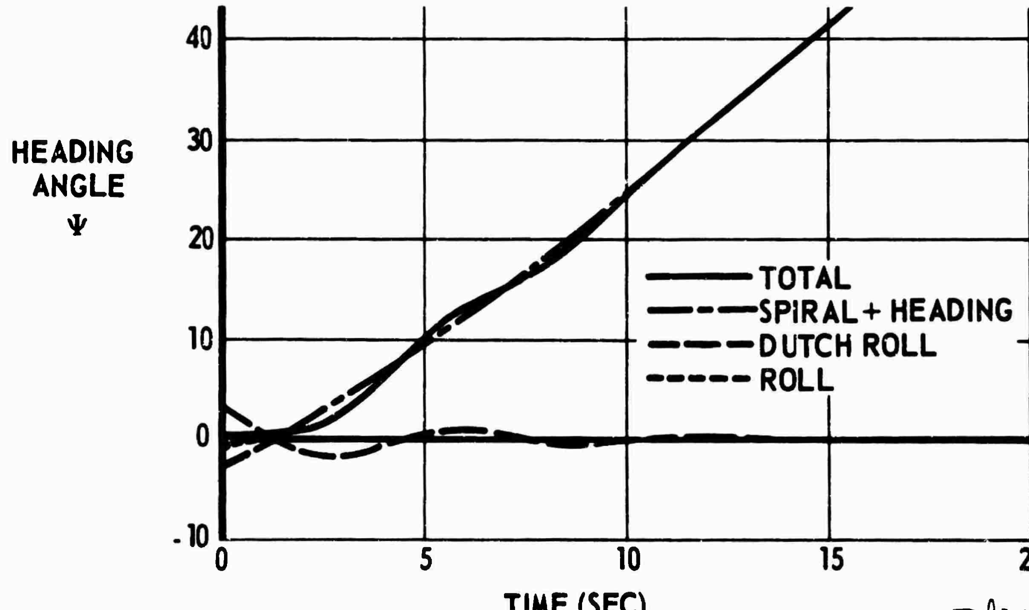
FIG. 1a



IMPULSIVE ADMITTANCE IN RESPONSE TO AILERON

DH CANADA 17-5

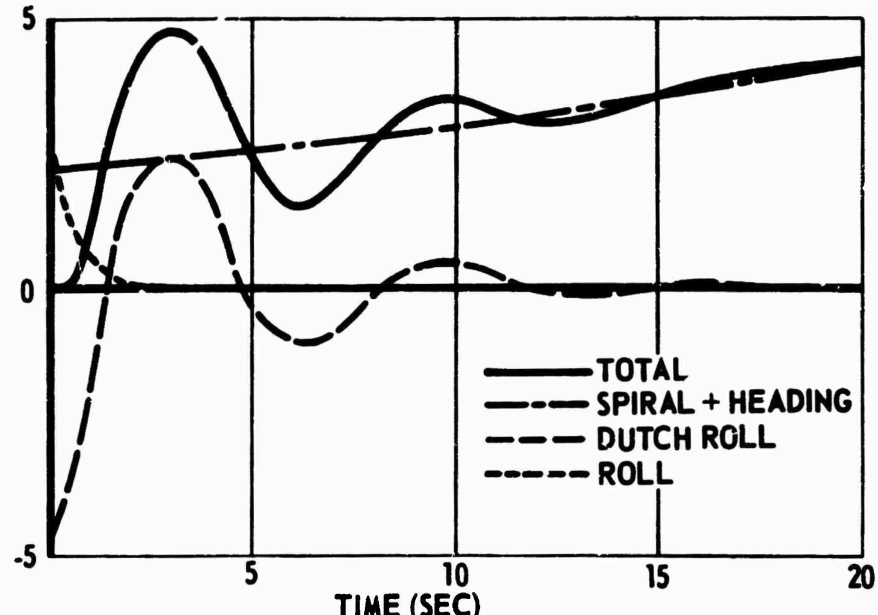
FIG. 1b



IMPULSIVE ADMITTANCE IN RESPONSE TO AILERON

DH CANADA 17-6

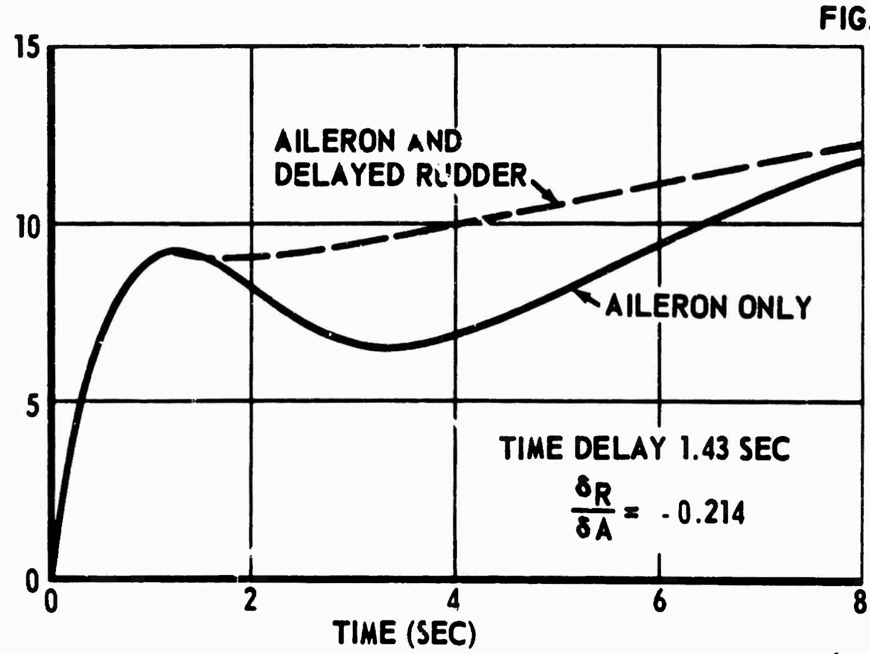
FIG. 1c



IMPULSIVE ADMITTANCE IN RESPONSE TO AILERON

DH CANADA 17-7

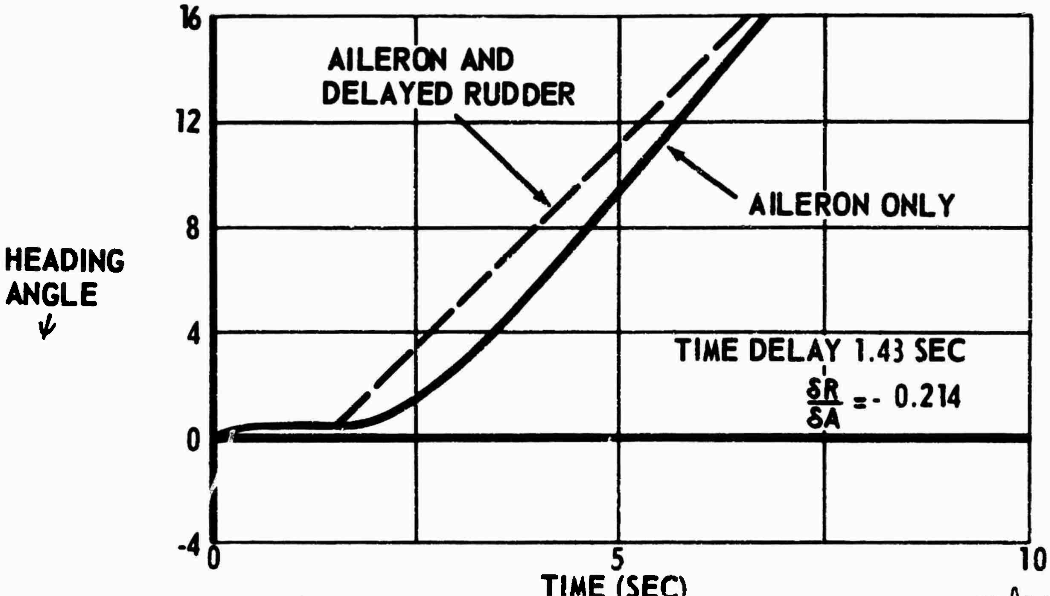
FIG. 2a



IMPULSIVE ADMITTANCE FOR RUDDER AND AILERONS PHASED TO ELIMINATE THE DUTCH ROLL

DH CANADA 17-8

FIG. 2b



IMPULSIVE ADMITTANCE FOR RUDDER AND AILERONS PHASED TO ELIMINATE THE DUTCH ROLL

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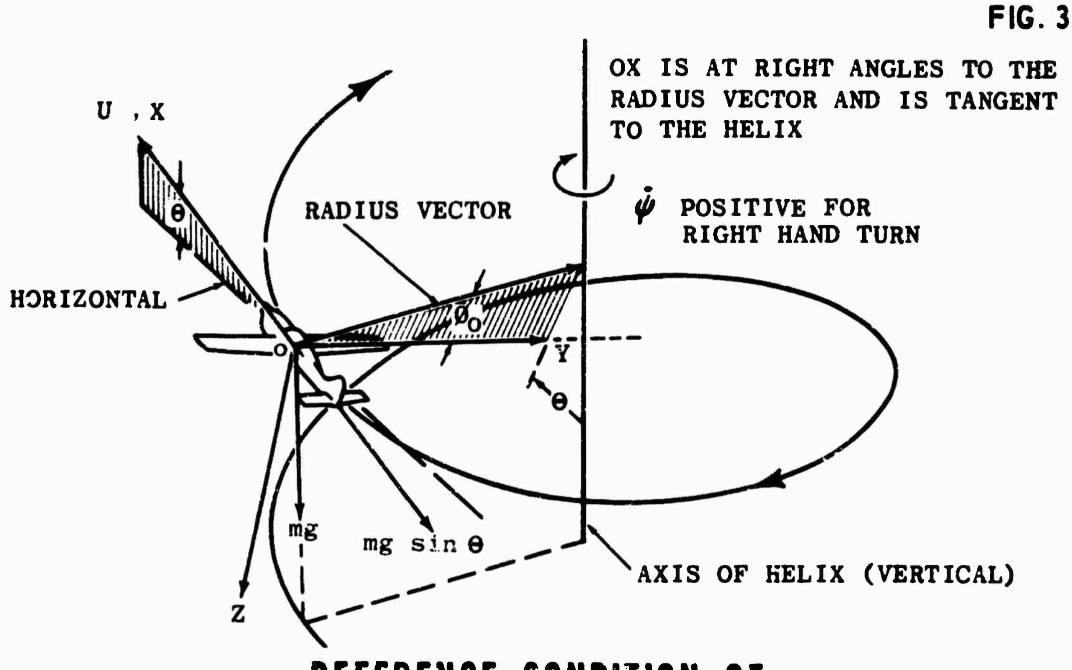
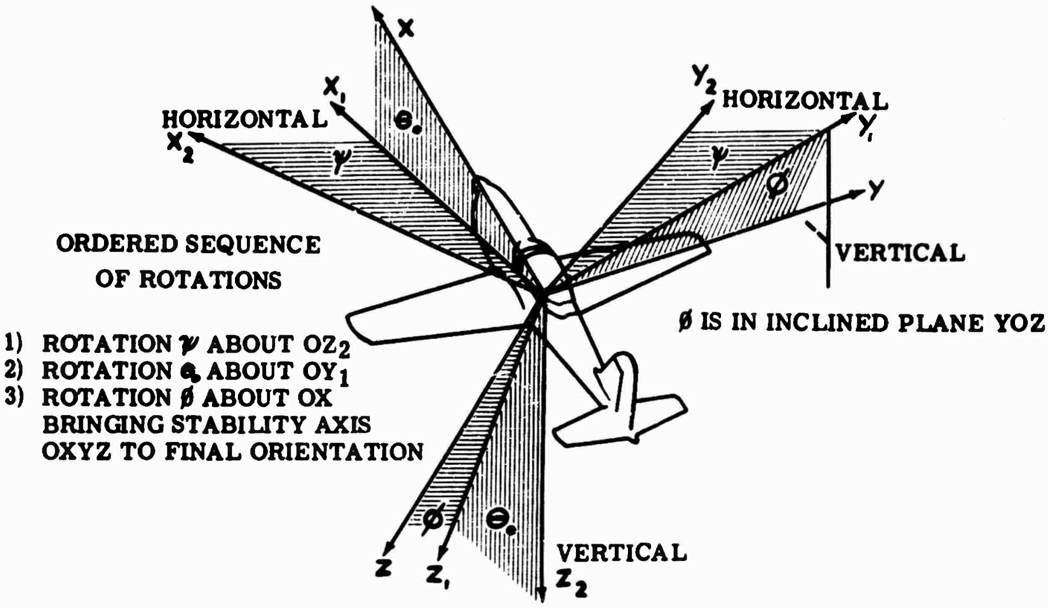
FIG. 3



REFERENCE CONDITION OF STABILITY AXES IN STEADY CLIMBING TURN

DH CANADA 17-10

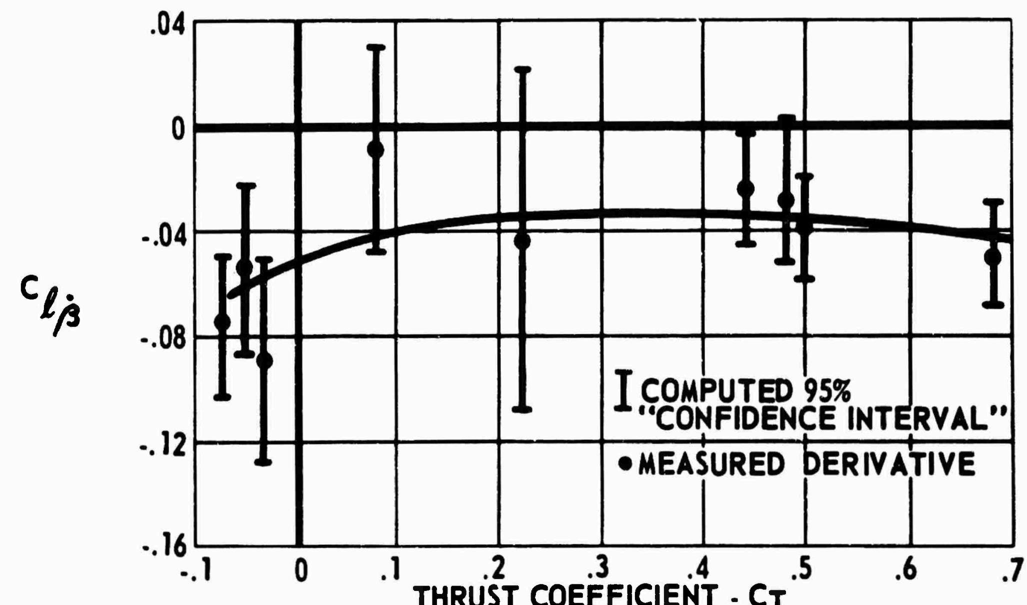
FIG. 4



RELATIONSHIP BETWEEN STABILITY AXES OXYZ AND EARTH FIXED DATUM AXES $OX_2\ Y_2\ Z_2$

17-11

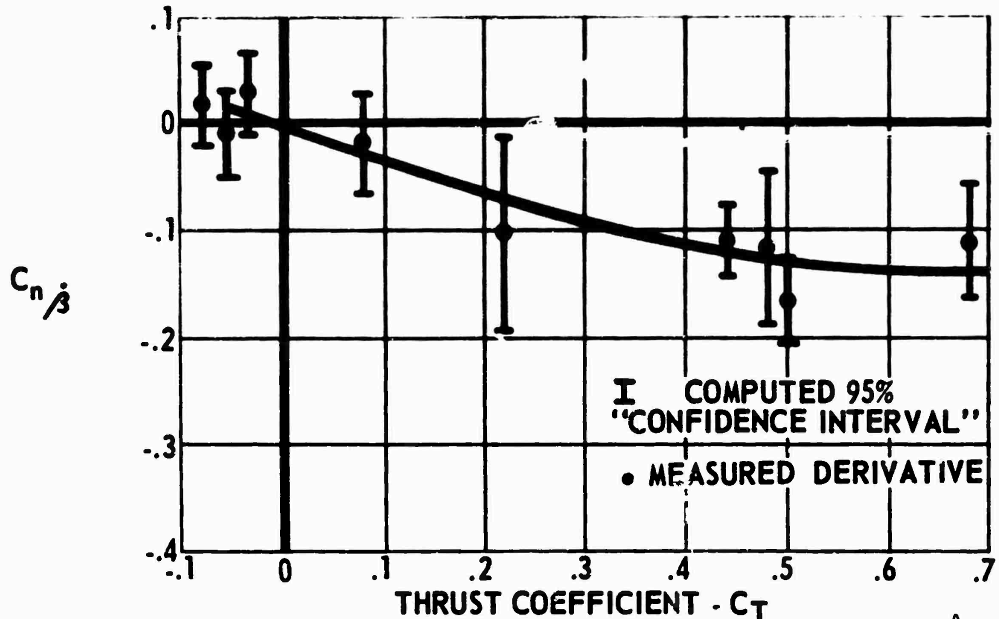
FIG. 5



AERODYNAMIC LAG DERIVATIVE MEASURED IN FLIGHT

17-12

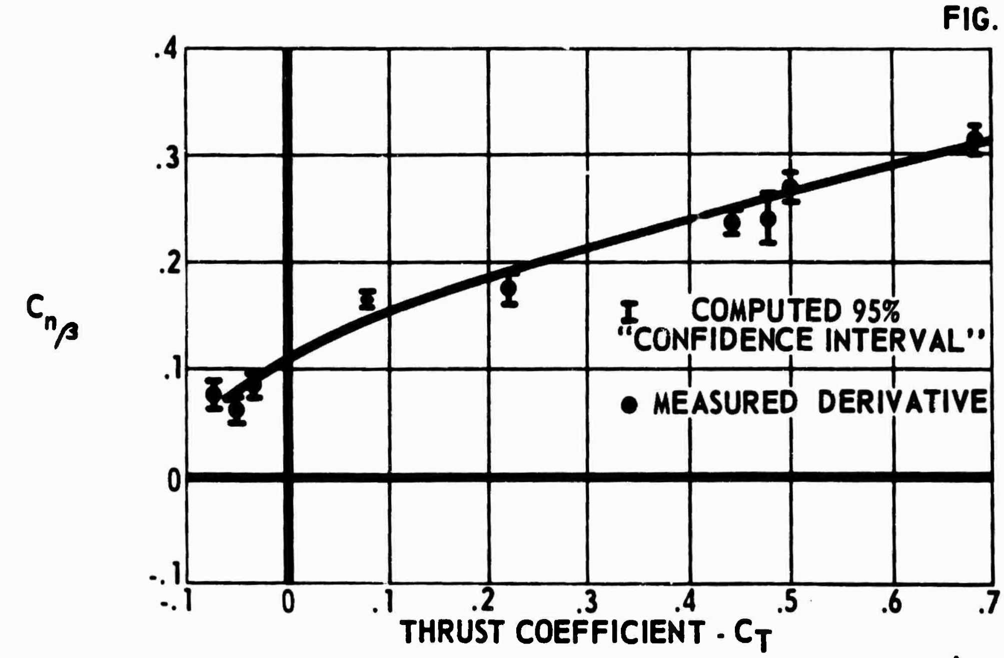
FIG. 6



AERODYNAMIC LAG DERIVATIVE MEASURED IN FLIGHT

DH CANADA 17-13

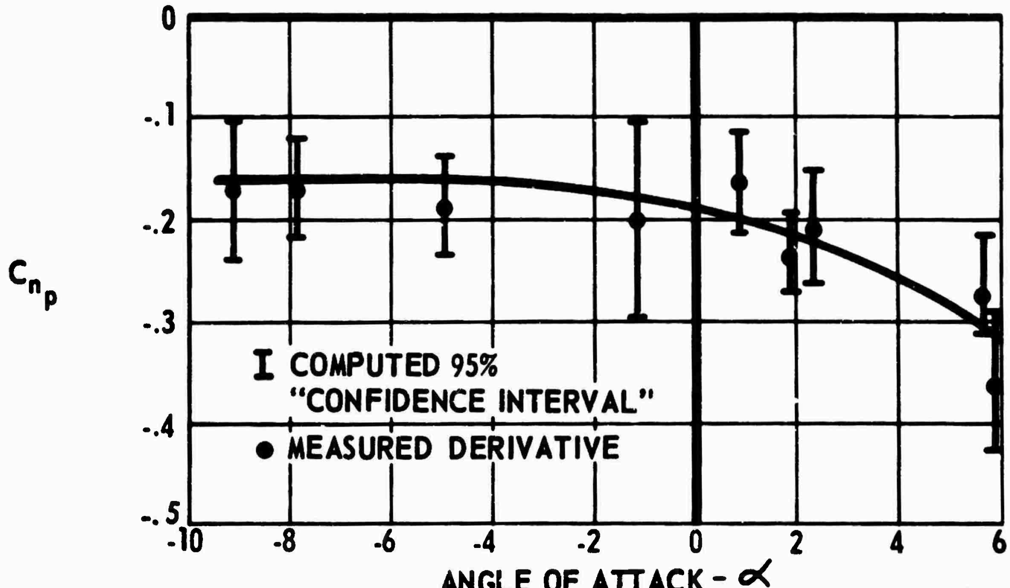
FIG. 7



STABILITY DERIVATIVE MEASURED IN FLIGHT

DH CANADA 17-14

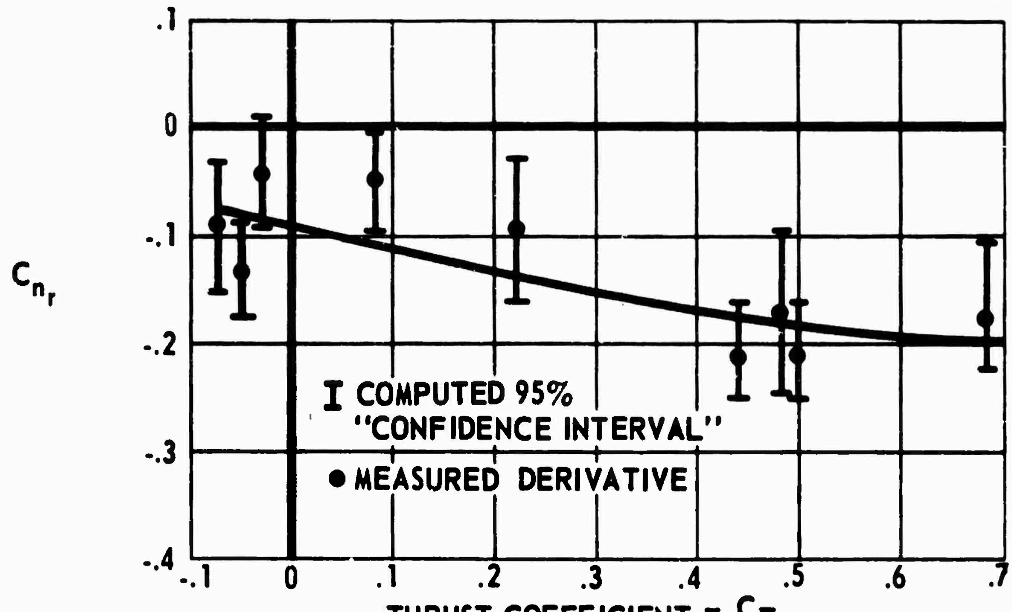
FIG. 8



STABILITY DERIVATIVE MEASURED IN FLIGHT

17-15

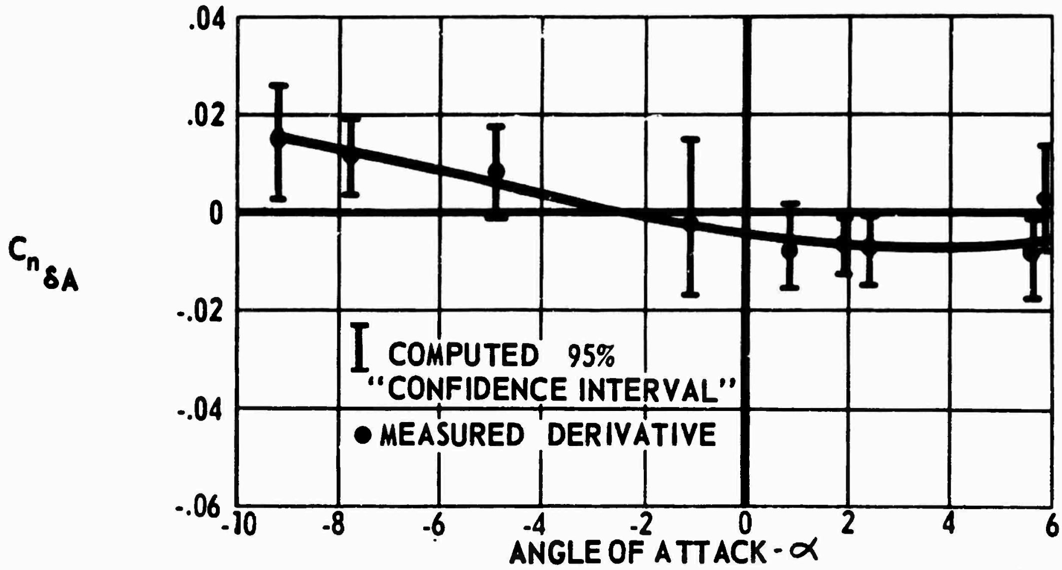
FIG. 9



STABILITY DERIVATIVE MEASURED IN FLIGHT

17-16

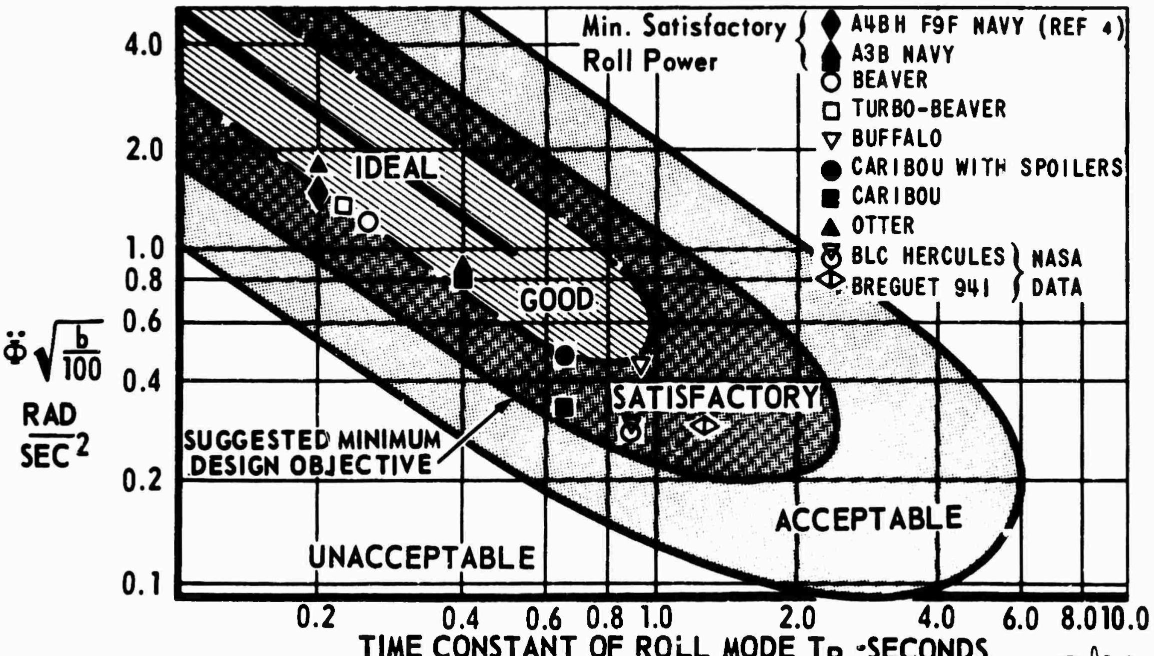
FIG. 10



CONTROL DERIVATIVE MEASURED IN FLIGHT

DH CANADA 17-17

FIG.11



SUGGESTED REQUIRED ROLLING ACCELERATION $\ddot{\Phi}$

DH CANADA 17-18

AERODYNAMIC RESEARCH - IMPROVEMENTS OF THE TILT WING CONCEPT

by

O. E. MICHAELSEN

Canadair Limited

1.0 INTRODUCTION

Aerodynamic research on the tilt wing, deflected slipstream V/STOL aircraft concept has been in progress for about fifteen years. Although sufficient useful data has been generated in this period to permit design and construction of potentially operational aircraft utilizing these principles, critical examination of the flight characteristics of these aircraft indicate several areas in which more research is needed in order to ensure the operational success of current and future aircraft of this type. A brief outline of these areas, as seen from the experience to date on the CL-84, is given below.

2.0 THE HOVER REGIME

The hover regime is loosely considered as that in which the resultant wind vector is less than 30 knots in any direction.

2.1 Free Air Hover

The free air hover performance of any VTOL aircraft is clearly of prime importance since it determines the disposable load. The "brute force" approach is undesirable for reasons too numerous to mention here. The prime index of hover performance of the tilt wing aircraft is the propeller figure of merit, and additional support for systematic experimental and theoretical research work, aimed at improving propeller static thrust, is needed. (Refer the first Technical Session of this Symposium). Overall aircraft "figure of merit" can be determined reasonably easily in flight. Since there is evidence that the aircraft figure of merit is a few percent less

than would be expected from isolated propeller, engine and transmission system test results combined with estimates of slipstream drag and interference, it appears prudent to recommend accurate thrust and torque measurements at the propellers of at least one available aircraft in hover.

The many apparent advantages of the mono-cyclic propeller demand extensive research on such propellers. However, since the control power available from these propellers may prove marginal, and the thrust loss, with full control, considerable, optimization of the aircraft configurations considered for utilizing mono-cyclic pitch propellers may prove essential in order to realize an overall advantage.

Stability, control and handling qualities of tilt wing aircraft in free air hover have received considerable attention in the past five years. In general, actual requirements for control power, control sensitivity and damping in the pitch and roll axes are considerably in excess of specified minima due to the dominance of the speed stability terms inherent in these configurations. Since these speed stability terms strongly influence the handling qualities of the aircraft in hover under gusty and turbulent conditions, it would appear important to investigate means of reducing these terms. How this can be achieved by aerodynamic means without seriously affecting low speed performance is not known by the author.

2.2 Hover in Ground Effect

Ground effect on hovering performance is generally favorable at near-zero airspeeds for tilt wing configurations. Although slightly adverse effects may be experienced at speeds above about 15 knots, these effects do not appear important since the power required is less than that for hover in still air. However, the effect of slipstream recirculation on stability, control and handling qualities, when "transitioning" near the ground or while hovering in a strong, gusty wind, do appear appreciable and may impose limitations on the operational use of the aircraft. It is believed that in addition to full scale flight testing, data from powered models, using mobile rigs or tracks, could prove useful in understanding these recirculation effects and aid in defining optimum flight procedures near the ground.

3.0 THE TRANSITION AND STOL REGIMES

For the purpose of this discussion, the transition regime is defined as the speed regime between hover and the wing fully down design speed, in free air. The same speed regime with ground effects is referred to as the STOL regime.

3.1 The Transition Regime

The steady state, symmetric flight, aerodynamic characteristics of tilt wing, deflected slipstream configurations comprise the major portion of available test data. Nevertheless, more such test data are required on practical "variable geometry wing" layouts in order to enhance the top speed potential of the tilt wing. It is well proven that adequate deceleration-descent margins during inbound transition can be obtained if the wing chord to propeller diameter ratio is kept above certain values. However, the resulting wing size is too large for efficient cruise at high speed. Although leading and trailing edge flap deflections improve the characteristics, large gains appear only possible if these devices provide large chord extensions with moderate deflection, particularly if adequate lateral-directional control in short landing approaches is to be maintained. Use of an exposed, high-speed cross-shaft for slipstream separation suppression should also be investigated. Such a scheme could prove particularly attractive in combination with mono-cyclic propellers where the cross-shaft location is less restricted than for a tail propeller controlled aircraft.

With regard to stability, control and handling qualities in transition, the field is so large that recommendations for general research are difficult. To date, the problems appear less significant than believed a few years ago, particularly with respect to lateral-directional control cross couplings. It may prove beneficial to investigate the effects of the speed stability terms on the handling qualities in the transition regime during extreme maneuvers, such as the turn-around maneuver, but some operational evaluation with existing aircraft should precede such an investigation to demonstrate if a serious problem exists.

3.2 The STOL Regime

Some of the research required in this regime is indicated in the above paragraphs. It is perhaps in this regime that the speed stability terms may prove most embarrassing, particularly during cross-wind approaches. Simulation in conjunction with flight test should be encouraged in this area.

4.0 THE CRUISE REGIME

Although existing tilt wing configurations suffer from several minor deficiencies in the cruise regime, it is difficult to recommend general research in this area. Perhaps aerodynamicists will pay more attention to this regime now that the hover and transition problems are better defined than they were when the current projects were initiated.

5.0 CONCLUDING REMARKS

In conclusion, the author wishes to express the opinion that the main aerodynamic problems of the tilt wing, deflected slipstream aircraft concept are reasonably well known and, in fact, satisfactorily solved in such aircraft as the XC-142 and the CL-84. Operational evaluation of these aircraft should do much to define the areas where aerodynamic improvements are needed.

AERODYNAMIC PROBLEM AREAS OF V/STOL AIRCRAFT AND RECOMMENDED RESEARCH

by

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INTRODUCTION

It is becoming clear that continuing advanced developments of V/STOL aircraft will eventually result in their fulfilling an important role in military and commercial transportation.

The increasing emphasis on V/STOL aircraft design developments is requiring expanded research efforts in various technical areas and, as a result, is causing an important resurgence of research efforts on aerodynamics and their problem areas pertinent to the V/STOL mission. The purpose of this paper is to outline and discuss several of the key areas of V/STOL aerodynamics which, from our experience, require careful analysis in V/STOL aircraft designs, and which would benefit from expanded research efforts. These are grouped under the following headings.

(1) Aero-propulsive Interference Effects

(2) Stall Characteristics

(3) Control Characteristics in a Propeller Slipstream

(4) Ground Effects

(5) Ducted Propeller Aerodynamic Characteristics

AEROPROPULSIVE INTERFERENCE EFFECTS

The subject of aeropropulsive interference effects introduces a variety of aerodynamic problem areas of V/STOL aircraft that would benefit from an expanded research effort. This subject is concerned with the interaction of the propulsion system flow characteristics with the inherent aerodynamics of the airframe and control surfaces.

One example of aeropropulsive interference effects is illustrated in the design of the X-22A V/STOL aircraft. It was realized that at intermediate duct angles in transition the downwash from the front ducts would produce large negative angles of attack at the rear lifting surfaces. Figure 1 shows the spanwise variation of downwash at the rear lifting surface assembly as visualized for the X-22A tandem duct configuration at a duct angle of 45°. This could produce nose up pitching moments, loss of aerodynamic lift, and the possibility of lower surface stall at small negative angles of attack. In the absence of reliable analytical or empirical design methods to accurately predict the downwash characteristics, these possibilities were prevented, by providing positive incidence and camber of the rear wing panel based on a series of powered wind tunnel model tests. In addi-

tion, a spanwise incidence distribution of wing, duct and stabilizer was provided in conventional flight to provide a favorable spanwise loading, which accounts for the downwash distribution $d\epsilon/d\alpha$ in conventional flight which is also shown in Figure 1.

Another example of aeropropulsive effects which have a direct bearing on control power requirements are the pitching and rolling moments induced by the inlet and efflux flow of the main lift propulsion units. Most of the current test data pertaining to these effects relates to specific configurations and is insufficient to permit separation of the moments due to the inlet and the efflux in terms of their dominant parameters.

Inlet pitching moments are produced at forward speeds by the momentum change at the inlet, and are reflected by high suction pressures over the windward inlet surface together with less negative or even positive pressures on the leeward surface.

Efflux pitching moments are due to negative pressure areas acting on surfaces downstream of the issuing jet and are induced by interaction of the free stream with the efflux flow. Their magnitude again depends strongly on the jet arrangement relative to the complete aircraft configuration.

Rolling moments due to engine inlet and efflux flow arise from similar considerations and occur under sideslip condition. They are caused by jet induced suction pressures acting on the

leeward wing and fuselage surface and can be large even at low to moderate angles of sideslip. Wing and engine placement have a strong influence on the size of the efflux induced rolling moments.

A further aspect of jet induced moments is concerned with reaction jets located at the wing tip for control. At moderate sideslip angles the negative pressures induced on the leeward side of the jet can reduce the controls efficiency significantly.

In the areas just described, which are typical of aero-propulsive interference effects, systematic studies are necessary for the development of design methods so that control requirements can be clearly defined and adequate control levels provided.

STALL CHARACTERISTICS

An aerodynamic problem area associated with certain types of V/STOL aircraft is that of primary lifting surface stall during lower power descents in the transition flight range. V/STOL aircraft types like the X-22A and tilt-wing aircraft achieve a varying portion of total lift in transition by a local mix of free stream and propulsive induced flow acting on the aerodynamic surfaces. While the surface angles can be large in transition, the local flow angle-of-attack is normally within the unstalled range. However, at low power conditions, such as in

descents or in decelerating flight conditions, the local angle-of-attack becomes dominated by free stream flow and stalled conditions can occur. In the case of the ducted propeller X-22A aircraft, this problem relates to duct stall.

There are two major sources of duct stall. One is stall of the upper outer duct surface and the other is stall of the lower inner duct surface. Test experience shows that in the flight regions where stall can occur, the net lift loss is not large; however, stall gives rise to two other significant problems. The first is associated with the onset of outer surface stall. Stall onset does not normally occur simultaneously on each duct; therefore, undesirable rolling moments are produced by the small unsymmetrical lift loss at stall onset. Free flight model testing of the X-22A showed that when upper surface stall becomes more fully developed on each duct surface the roll disturbances tend to disappear. A typical variation of duct normal force coefficient with duct angle at a low thrust coefficient is shown in Figure 2 based upon 1/5 scale model tests. The effect of upper surface stall is shown by the small but distinct shift in the level of normal force coefficient. It is important to note that in model test evaluations of stall characteristics, scale effect is a very important factor. Small scale test results can indicate stall at substantially lower angles of attack than full scale and may give erroneous indications of the type of stall progression that occur in full

scale. On the 1/5 scale X-22A stability model, it was necessary to add an enlarged lip radius on the lower duct surface in order to prevent stall at high duct angles such that unstalled stability data could be obtained. A photograph of the 1/5 scale duct with the large lip radius is shown in Figure 3. The second problem develops from the case of lower inner lip stall. The problem is one of propeller blade stresses and associated vibrations created by the unsteady blade loading. Proper design of the duct section profile can insure that stall - particularly deep stall - is outside a reasonable operating flight envelope. However, further research effort is needed to develop practical means of delaying stall, such that wider operational envelopes will be available.

CONTROL SURFACE CHARACTERISTICS IN A PROPELLER SLIPSTREAM

A further important area of investigation in V/STOL aerodynamics is that of control. Attitude control for the hover and low speed flight regimes can be provided in a variety of ways. These include reaction controls, thrust modulation, auxiliary propulsion units, and aerodynamic surfaces mounted in the propeller slipstream. The latter method presents a very effective means of obtaining control in a propeller slipstream; however, the development of an efficient control surface from the standpoint of effectiveness and power requirements is a difficult problem. The difficulty of determining the aerodynamic characteristics of these control surfaces is due to the

complex flow situation and its variation with surface deflection. This can best be illustrated by considering the X-22A elevon control surfaces.

The X-22A has an all movable aerodynamic control surface (elevon) mounted in each duct. These surfaces provide yaw control during hover flight and are phased to produced pitch and roll control during cruise flight. The problems of predicting aerodynamic characteristics of the control surfaces vary according to the three flight regimes (i.e., hover, transition, and cruise) however, the greatest difficulty is in hover and transition.

In hover and transition, the thrust coefficient is high and the flow in the duct is characterized by a large amount of propeller swirl. The swirl produces induced angles of attack on the surface which differ in direction from one side to the other. As the surface is deflected, one side tends to zero lift whereas the other side increases in lift resulting in a non-uniform span-wise loading. The X-22A elevon is designed with a horn balance on one side to minimize hinge moments. The induced angles and therefore the loads on this side of the elevon are generally larger than those on the other side because the leading edge is close to the propeller plane. A further problem associated with predicting the control effectiveness is the decrease in dynamic pressure between the trailing edge of the duct and the trailing

edge of the elevon. Aerodynamic tests to determine the control effectiveness of the X-22A elevons indicated that the dynamic pressure decreases approximately 30% between the duct trailing edge and the elevon trailing edge.

Powered model wind tunnel tests are presently the only reliable means of estimating control effectiveness in a propeller slipstream. Figure 4 presents a comparison of the control effectiveness of three control configurations mounted in the exit of a duct. The data presented was obtained from a wind tunnel test conducted at Bell Aerosystems. The flow in the duct was supplied by a centrifugal blower and; therefore, the results do not indicate the effect of propeller swirl on control effectiveness. The data does, however, present the relative increase in effectiveness from one configuration to the other. The data indicates that an elevon with a compound hinge (two hinges located approximately at the .25 and .75 chord line) approximately doubles the control effectiveness of a plain elevon of the same planform having a simple hinge located at the .25 chord line. It also shows that the compound elevon when configured in a tri-plane arrangement (i.e., with two auxiliary vanes, one mounted above and the other below the main surface) is approximately three times as effective as the plain elevon. Further research is desired, both tests and analytical, to investigate the control effectiveness characteristics of various types of surfaces located in a propeller slipstream.

GROUND EFFECTS PROBLEMS

The current high degree of interest in V/STOL aircraft demands a greater understanding of the operational problems peculiar to this type of aircraft. During the first phase of VTO operation or near the ground, a V/STOL aircraft is exposed to one or all of three basic ground effect phenomena as illustrated in Figure 5; (1) Hot Gas Recirculation, (2) Induced pressures created by the ground deflected exhaust flow, and (3) Ground Erosion. All these phenomena are produced by the high energy exhaust flows. In the case of a jet V/STOL aircraft, the jet efflux strikes the ground and spreads radially in a relatively thin sheet until turbulent mixing and convective forces cause it to thicken and rise around the aircraft. This mixture of ambient air and hot exhaust gas can be ingested by the jet engine inlets resulting in a loss in engine performance. Figure 6 shows the adverse effect of forward facing side inlets compared to top inlets and indicates that top inlets should experience a maximum of from 5 to 15 degrees rise in air temperature. Figure 7 shows the adverse effect of headwinds, with and without aft vectoring of the jet efflux. For this reason, problems of hot gas ingestion are expected to be encountered during ground rolls and, more specifically, during STOL. Controlled continuous vectoring of the exhaust can assist in reducing the inlet temperature rise.

The mixture of air and hot exhaust gas also causes

localized heating of aircraft components. In model tests it was found that temperature levels as high as 300 to 400°F above ambient can be expected in local areas on the model. Judicious location of temperature sensitive components and localized use of temperature resistant alloys can generally preclude severe surface heating problems. Reference 2 provides a technique based on test data for estimating the air temperatures in the vicinity of a jet V/STOL aircraft and a basis from which to conduct a heat transfer analysis to determine surface temperatures.

The second phenomena, induced pressures, can produce a substantial lift loss resulting from the entrainment of the relatively still air beneath the aircraft through turbulent mixing with the radially flowing ground flow jets. This interference phenomena is highly configuration dependent and does not lend itself readily to precise analytical definition. Thus, model testing is essential.

Bell has developed a empirical method (Reference 3) for estimating the induced lift loss for jet V/STOL aircraft. The method consists of a set of design charts that are a function of ground height, ratio of planform area to jet efflux area, jet separation distance, and wing height. These charts have been verified from X-14A and US/FRG model test data. Tests recently conducted by Bell using a model configuration with separated

multiple jets resulted in developing a means of substantially reducing induced lift loss and pitching moments in ground effect. This means was a set of "ground effect doors" located in the vicinity of the "fountain" of vertically flowing exhaust gases which is caused by the mutual impingement of the ground flow jets. Figure 8 shows the variation of induced lift loss with height above the ground and shows the beneficial effect resulting from the use of the "ground effect doors". These doors are also beneficial in blocking the hot exhaust gases from ingestion by the engine inlets.

The induced pressures caused by V/STOL aircraft in ground effect is not always detrimental. A case in point of beneficial ground effect is the tandem ducted propeller arrangement of the X-22A. Here the "fountain" created by the duct efflux flow is such as to provide an increase in net vertical force in ground effect of as much as 13 percent of the net installed thrust out of ground effect.

While test data and empirical methods are gradually being developed to describe ground effect phenomena, it is recommended that research studies be conducted in the following areas:

(1) Analytical studies to determine the relative importance of the various dimensionless parameters which are used as correlation parameters in the study of Hot Gas Recirculation and

Induced Pressures. Experimental tests will be necessary to support this study. (2) Methods to predict wind tunnel wall effects pertinent to V/STOL powered model tests should be developed. (3) Scaling effects to define the application of scale model data to full scale vehicles should be studied.

DUCTED PROPELLER AERODYNAMIC CHARACTERISTICS

As an aerodynamic device the ducted propeller has received a relatively meager amount of study and experimental investigation. Model tests are desired to obtain systematic performance information as a function of the principal design parameters such as duct inlet geometry, duct length, duct exit geometry, propeller blade activity factor, blade section and twist distribution, and blade tip clearance.

Generalized design information on the thrust, drag and moments, of isolated ducted propeller units should be generated from the tests. A further extension of this information should cover performance in the presence of flow disturbances such as bodies, surfaces, other interacting propeller slipstreams, or jet exhausts. The performance in and out of ground effect should be investigated as well. The investigations should include the testing of control surfaces, deflector and struts in the duct slipstream.

The bulk of the wind tunnel investigations of ducted propeller characteristics have been in support of specific

configuration verification during the detail design phases of various aircraft. A one third scale model and a full scale ducted propeller model were tested for X-22A aircraft design verification. Results showed good agreement with the predicted static thrust values, as seen in Figure 9, but indicated that conventional flight thrust was lower than predictions as shown in Figure 10. Considerable flight thrust improvement can be obtained by continued aerodynamic development tests to refine the duct internal flow characteristics.

Much research work of this type remains to be accomplished with emphasis on performance improvement for units designed for other representative power loadings and duct configuration variations. A major objective of this activity should be to provide correlation data for development of more accurate analysis methods for computing the predicted performance of a given design.

To realize the full potential of ducted propeller propulsion systems for lift, thrust and control forces, a broad integrated research program should be organized and implemented. The resulting state-of-the-art advancements will have immediate applicability in the design of V/STOL aircraft and many other types of aerospace and surface level transportation systems.

1. Bell Aerosystems Company Report No. 2099-928003, "Generalized Experimental Study of Inlet Temperature Rise of Jet V/STOL Aircraft in Ground Effect," by R F. Speth and P. E. Ryan, March 1966.

2. Bell Aerosystems Company Report No. 2099-928004, "Model Tests and Analysis of the Temperature Environment Induced by Jet V/STOL Aircraft Operating Near the Ground," by R. F. Speth and P. E. Ryan, May 1966.

3. Bell Aerosystems Company, Interoffice Memo 354:65:1111-1:CES, "Induced Lift in Ground Effect for Jet V/STOL Aircraft," by C. E. Satterlee, November 11, 1965.

FIGURE 1. SPANWISE DOWNWARD DISTRIBUTION AT REAR ASSEMBLY

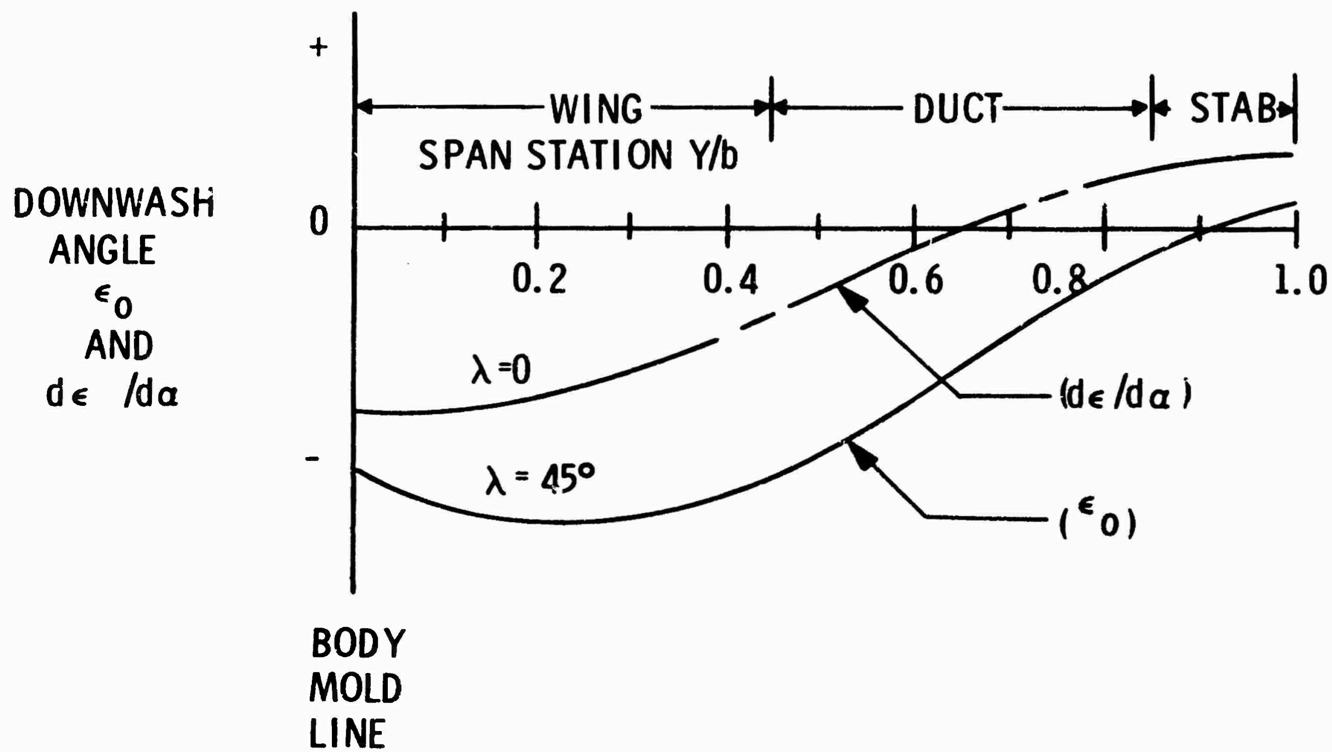


FIGURE 2. DUCT NORMAL FORCE VARIATION WITH DUCT ANGLE

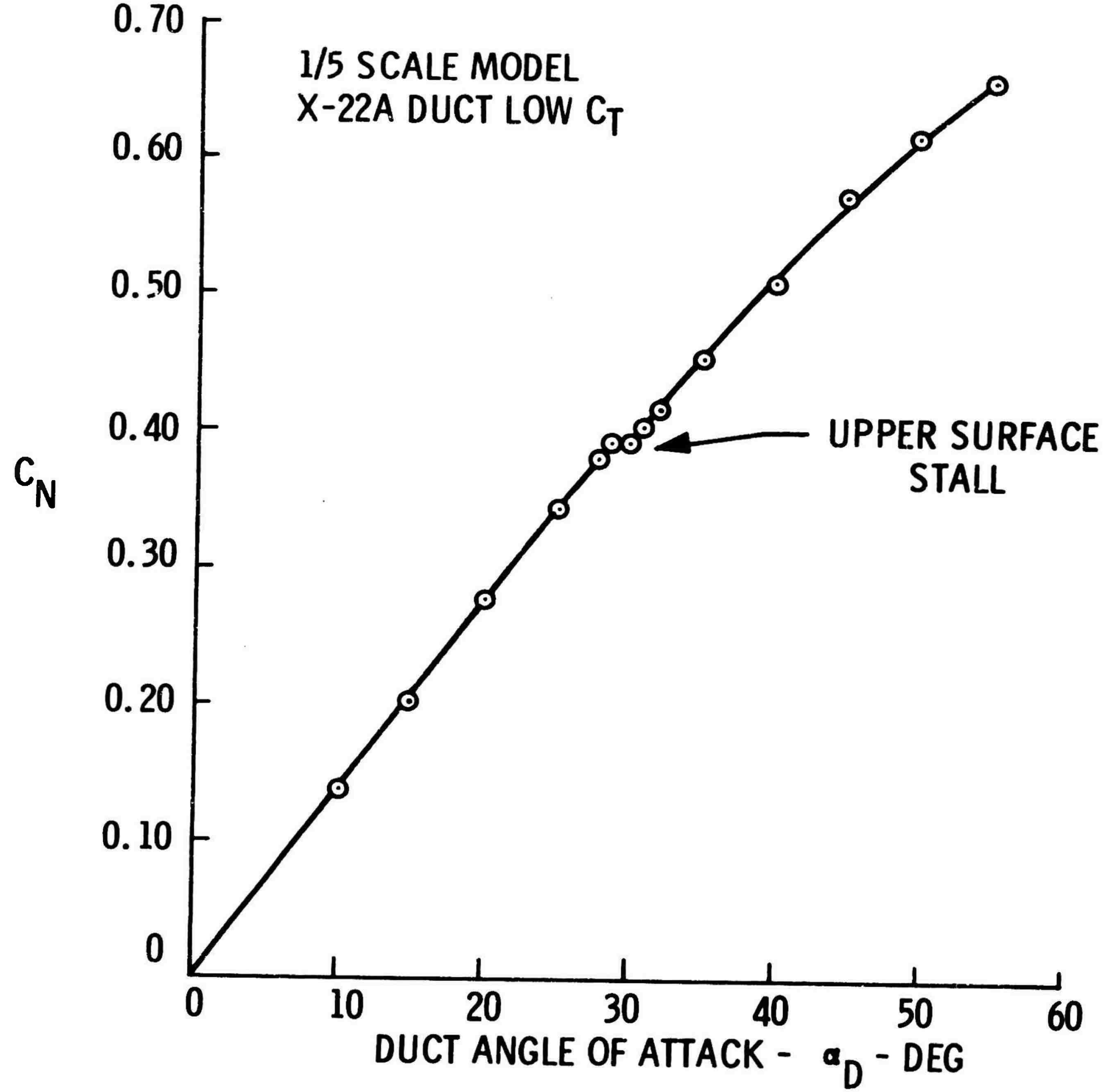


FIGURE 3. MODIFIED DUCT INLET ON ONE-FIFTH SCALE MODEL

FIGURE 4. COMPARISON OF CONTROLS EFFECTIVENESS

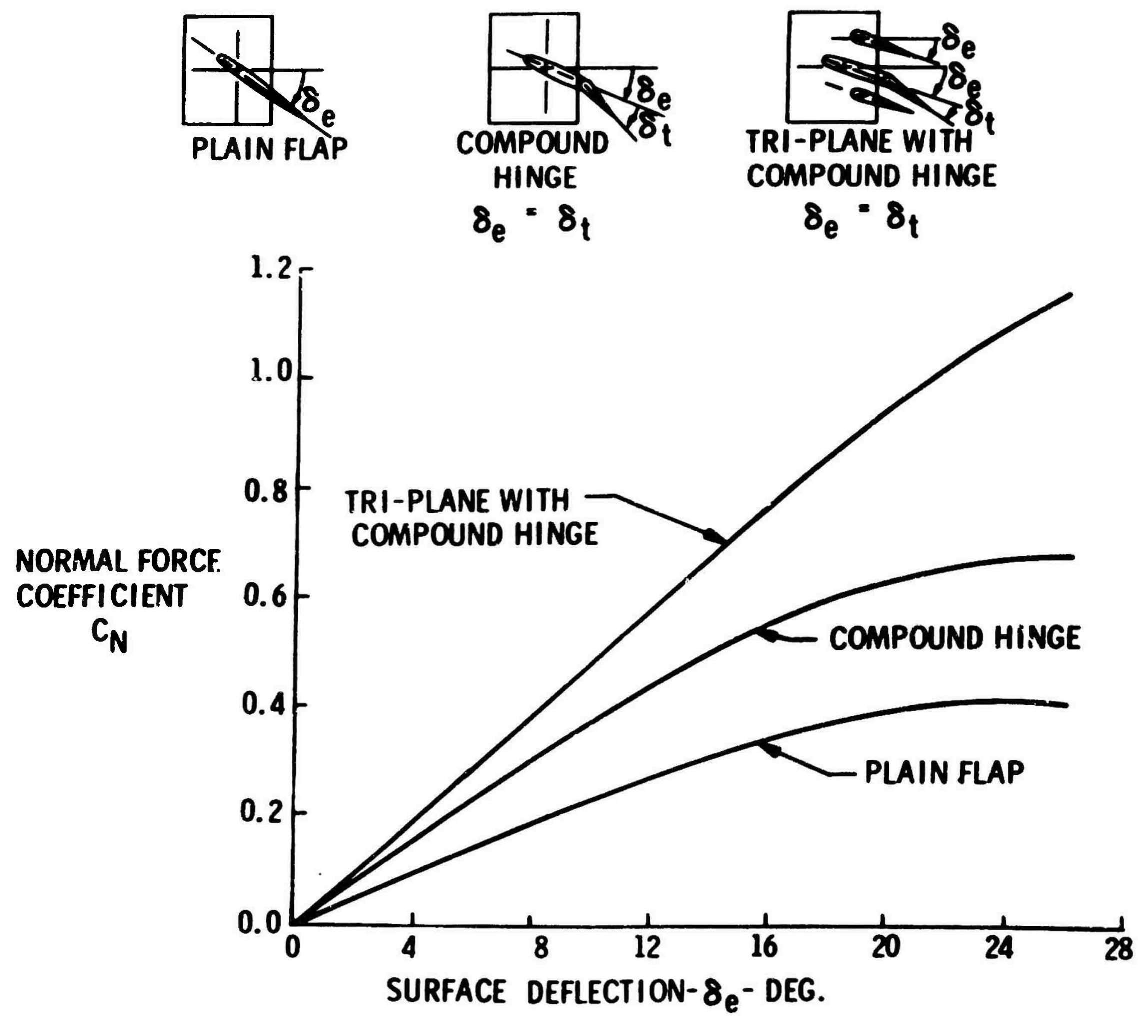


FIGURE 5. JET V/STOL OPERATION IN GROUND PROXIMITY

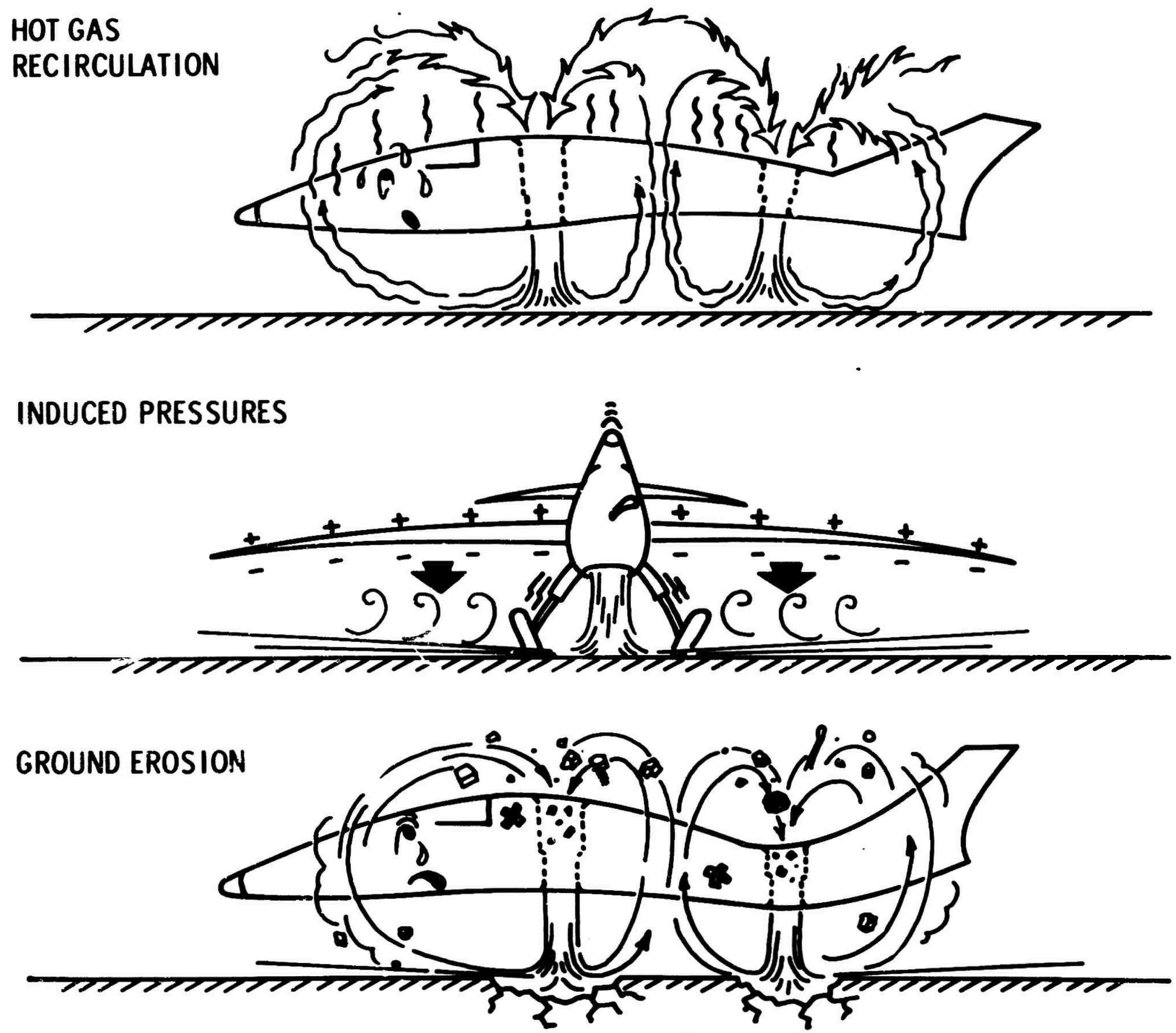


FIGURE 6. SIDE INLET EFFECT

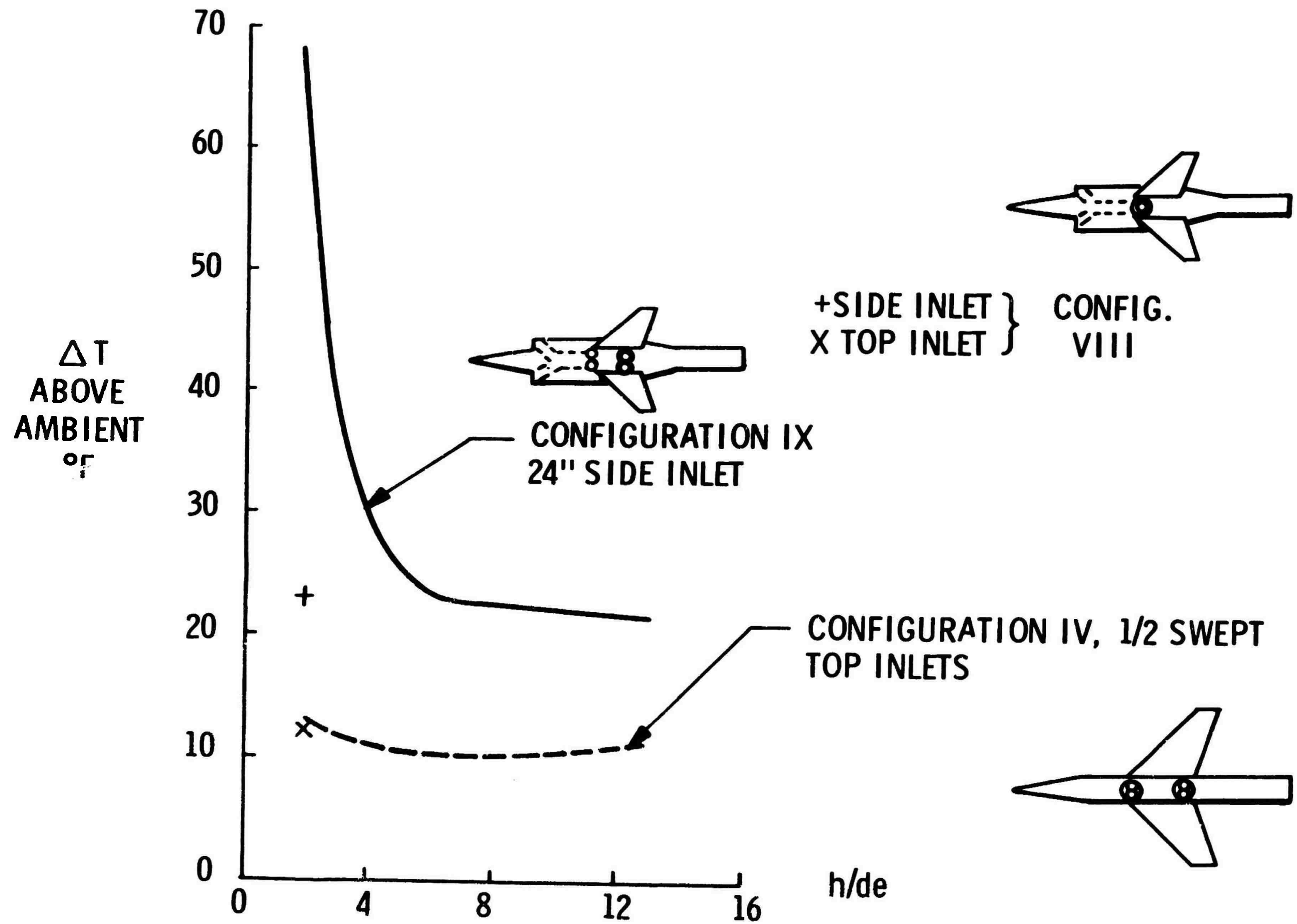


FIGURE 7. HEADWIND EFFECT AT TAKEOFF HEIGHT (h/d_e = 2)

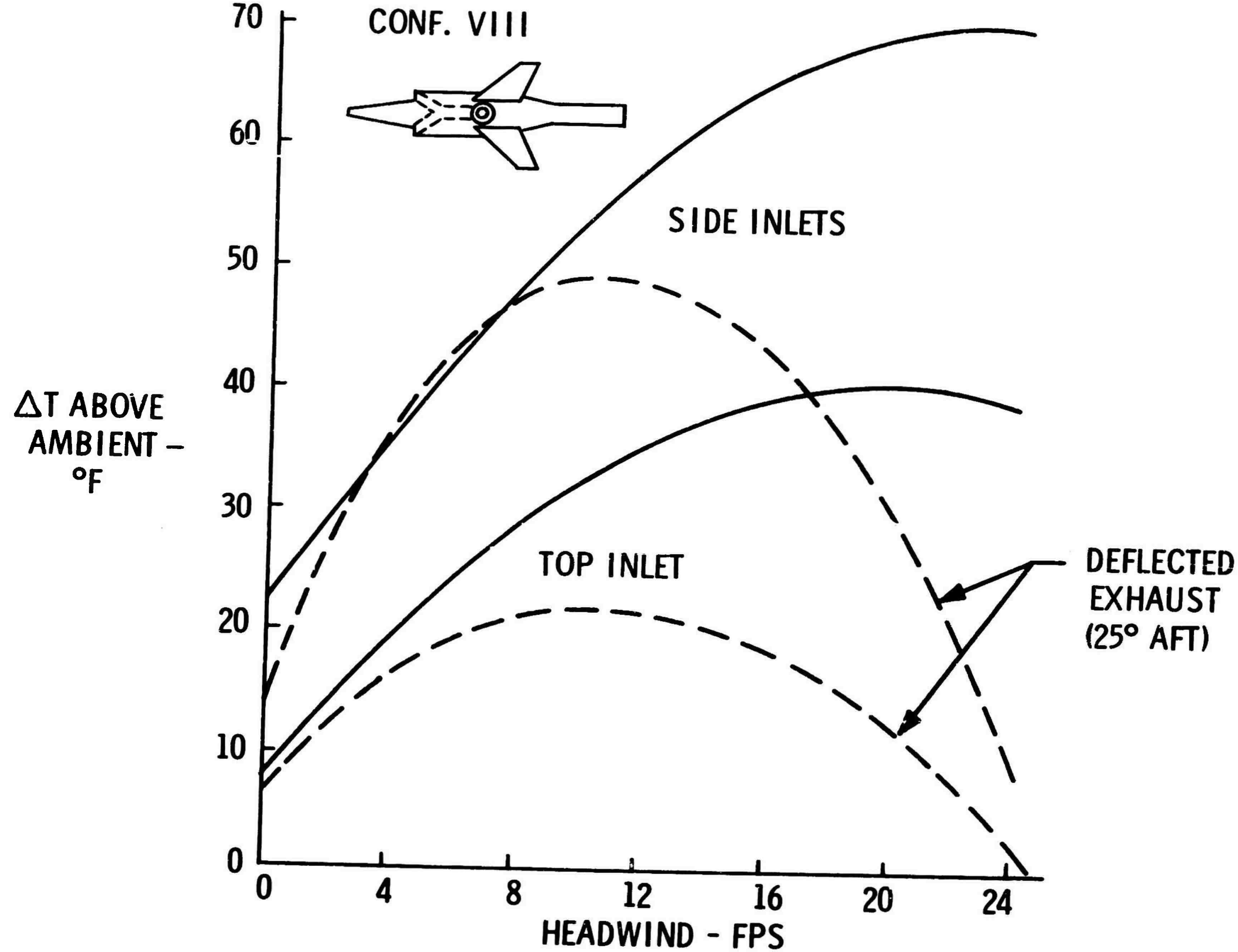


FIGURE 8. RESULT OF GROUND EFFECT DOORS

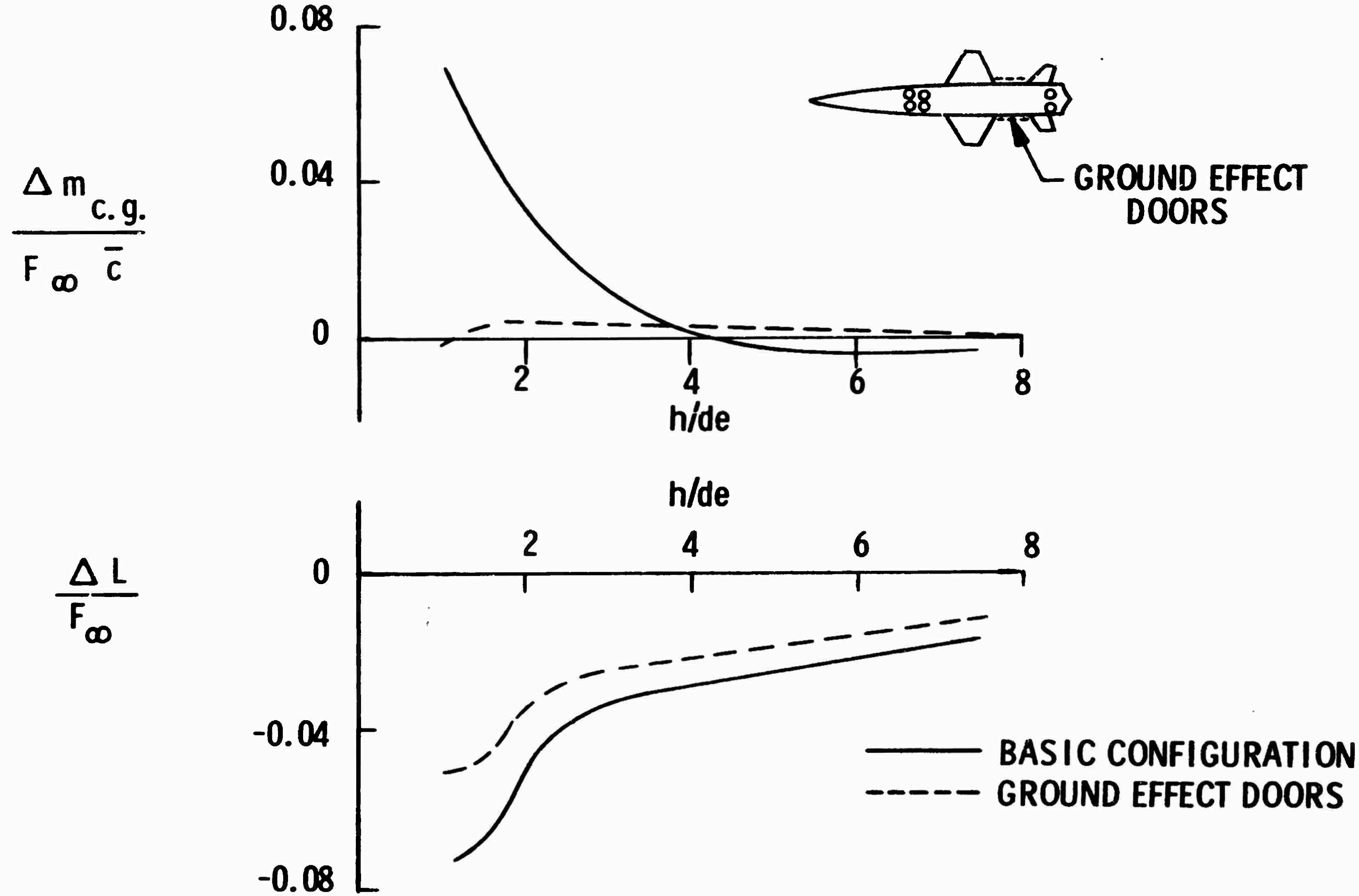


FIGURE 9. DUCTED PROPELLER PERFORMANCE STATIC FULL SCALE DUCT

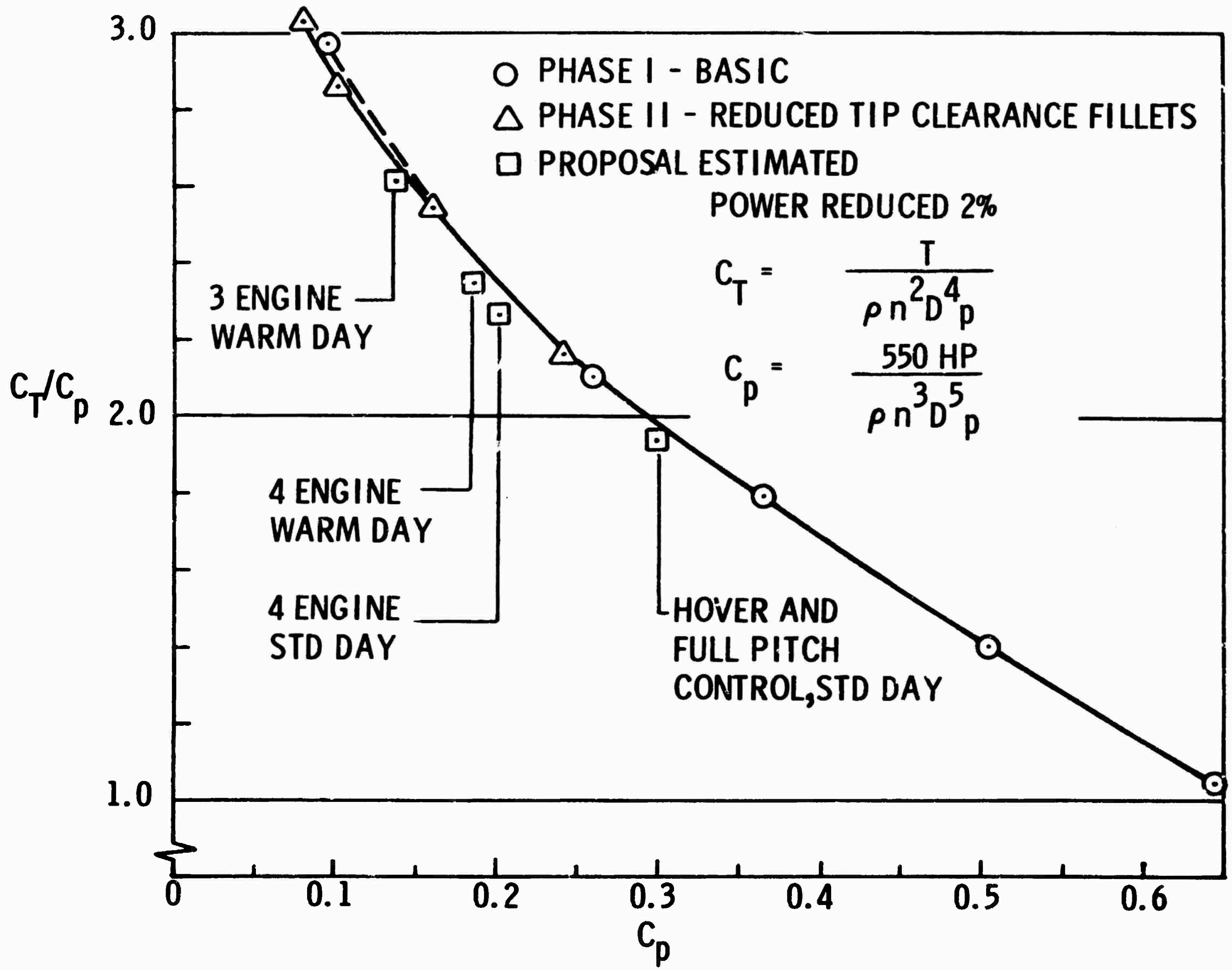
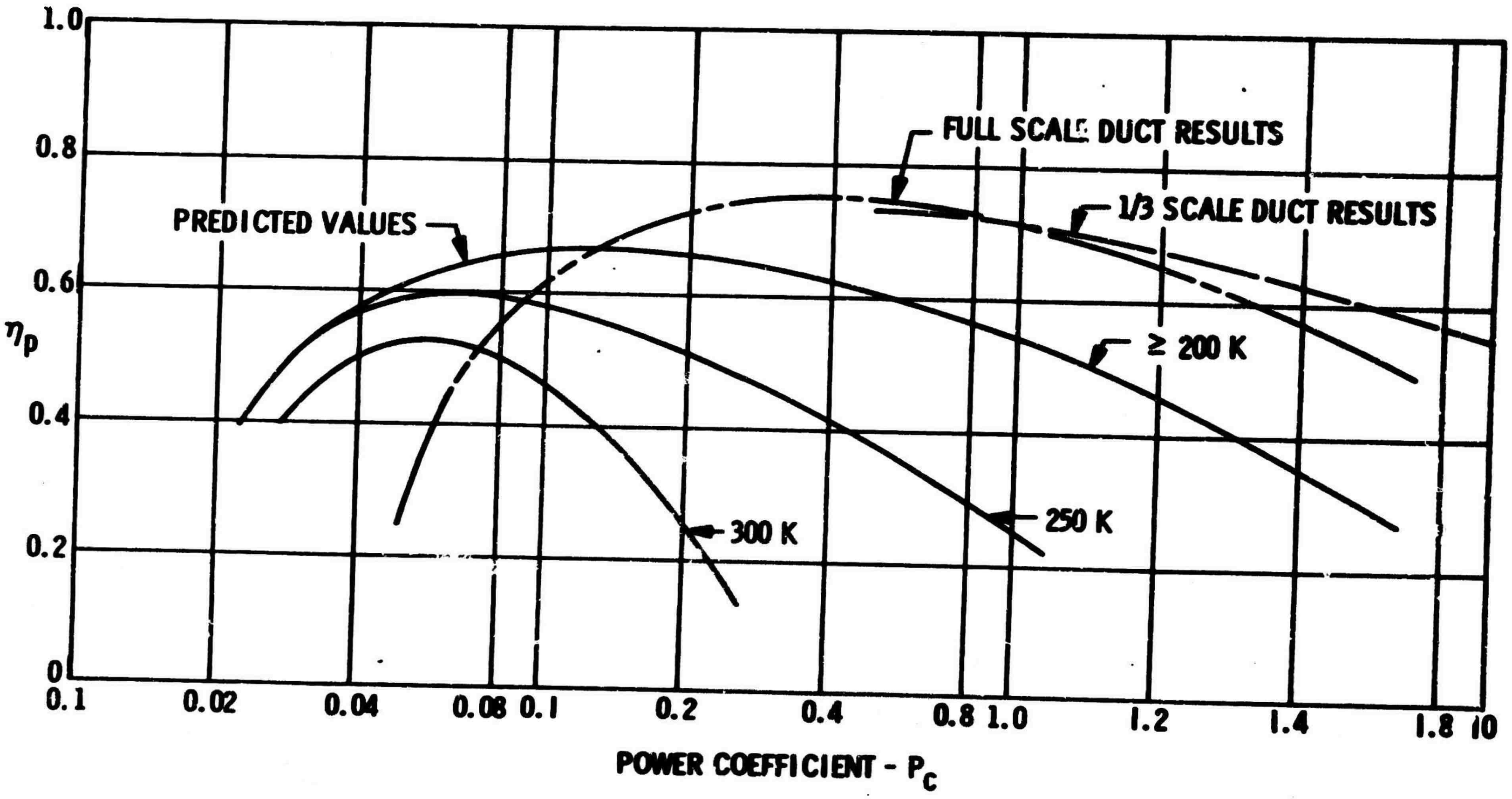


FIGURE 10. PROPULSIVE EFFICIENCY VS. POWER COEFFICIENT



A DISCUSSION OF LOW SPEED VTOL AERODYNAMIC PROBLEMS AND SUGGESTIONS FOR RELATED RESEARCH

by

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INTRODUCTION

The discussion of VTOL problems and suggestions for related aerodynamic research and development presented to this conference by LTV is principally an out-growth of the experiences gained during the design, development, and Contractor flight testing of the XC-142A Tri Service Assault Transport under Contract AF33(657)-7868, sponsored by ASZTV, ASD, AFSC, Wright-Patterson Air Force Base, Ohio, USAF. While LTV is engaged in the development of other VTOL concepts such as the high by-pass ratio fan concept (ADAM) described briefly in references 1 and 2, it is believed that the experiences related to XC-142A are most meaningful at the present time since they represent the practical results of a design cycle.

Most of the readers are by this time quite familiar with the XC-142A airplane, so that only a few details will be repeated here for reference. The XC-142A is a 38500 pound, four engine, four propeller, tilt wing cargo transport of generally conventional aircraft arrangement developed as a Tri Service

vehicle under the program management of the ASD of the U. S. Air Force. The vehicle was designed to be capable of vertical take off with a four ton payload. All four propellers and a pitch control tail rotor are interconnected by shafting.

Five of these aircraft were procured and three are presently in flight status and participating in various phases of the Air Force Category II Flight Program. A fourth airplane will return to flight status in July. These aircraft have accumulated over 300 flights for approximately 240 flight hours and, as of this writing, 27 military and civilian pilots have successfully and readily performed "verti-circuits" (vertical take off, conversion to cruise flight, reconversion to hover, and vertical landing). Many practical tests are now underway to define the full military potential of the airplane, such as cargo drops, high field elevation tests, unprepared field operations, assault transport operations, and operations from an aircraft carrier at sea. Although the XC-142A is not a perfected machine in its present form, we take pride in having developed the first VTOL airplane capable of performing a practical military mission and enabling the Military Services to realistically evaluate the concept.

In reviewing the XC-142A program for aerodynamic problems suggesting research, it is of interest first to briefly review the aerodynamic design areas that were considered to be of prime concern at the outset of the design program, and then discuss the status of these problems and their related technology today. To complete the picture, then, new problems which have been identified in flight tests are included and the research and development needed to improve our ability to solve and avoid similar problems in the

future is presented. Most of the discussion will obviously be centered around the tilt wing type of VTOL, but the comments made will frequently have bearing on the other concepts as well.

DESIGN DEVELOPMENT PROBLEMS

At the outset of the XC-142A design, the primary areas of concern aerodynamically were:

1) Static thrust
2) Decelerating or descending flight capability at low speeds
3) Stability and control in hover and low speed flight

The emphasis at LTV at that time was principally on development. We believed that we had shown in our proposal a vehicle basically capable of meeting its design requirements, and that the job ahead was one of detail design. Aerodynamically, feasibility was not the question; the problem was, and still is today, the accurate prediction of absolute capability and the effects of change, without resort to extensive wind tunnel test or even flight test. In many areas, a new tilt wing design will be still in jeopardy of over-design until both research and development provide better solutions.

Static Thrust

First, let us look at the problem of accurate prediction of static thrust. As is well known today, the propeller static thrust proved in flight to be substantially less than estimated. Even today, there is still further understanding required to fully explain this discrepancy in design performance. Frankly, although we knew the static performance goals for the XC-142A propeller to be tight, we believed the propeller weight objectives to be much more difficult to meet, considering the state-of-the-art of fiberglass blade

construction. On the other hand, propellers without any specific tailoring for static thrust had shown by whirl stand measurement performance sufficiently high to give confidence that the desired performance could be met. However, flight tests revealed the thrust discrepancy to be approximately 8% aerodynamically, and 2% additionally because of a structural restriction to 95% of design RPM. A comparison of predicted and flight performance is shown in Figure 1. The possibility of performance less than predicted had been indicated prior to flight by Wright Field Whirl Stand tests. A description of the general physical properties of these stands is included in reference 3. Four whirl stand configurations representing primarily differing amounts of blockage indicated propeller performance varying from predicted to approximately 10% low. The latter result was obtained on the No. 4 stand with a 14 foot extension, and represented the minimum blockage test. The tests on this stand were later repeated, confirming both the original and airplane results, and an emperical program was then conducted on this stand to achieve improved propeller performance. Eventually, a configuration was obtained which regained all but approximately 2% of the thrust originally expected at power conditions of prime importance. It is of particular interest to note that the geometric characteristics of this revised propeller are practically identical to the Super Constellation 15.156 foot propeller. The initial flight results with this propeller are compared with the whirl stand results and a fairing of flight data for the original propeller in Figure 2. Note that the flight results appear to agree well with the whirl stand tests. These results and the test results with the original propeller indicate that

for these propellers good correlation is possible between the test stand and the XC-142A flight tests for this particular stand configuration.

Further whirl stand tests are planned in support of the C-142B study program to achieve additional improvements in efficiency.

In view of our experiences, it is very gratifying to observe the theoretical and experimental activity presently under way to provide better methods of thrust prediction, both statically and at low speeds. At this point, rather than suggesting additional research, we make the following observations:

1) Whirl stand testing of the XC-142 propeller and other recent propellers indicates a significant sensitivity to blockage. Therefore, test data of propellers obtained on stand configurations with large amounts of blockage should be used with great caution when attempting to correlate theory and experiment for an isolated propeller. We suggest that the Whirl Stand data now available from XC-142 propeller tests may provide large scale propeller results as reliable as any in existence for comparison with theory.

2) Test facilities need development for statically testing the full scale or large scale propeller for performance purposes (as opposed to strength), which are free from unnatural airflow circulation but which can include natural blockage such as adjacent nacelle, wing, fuselage, and ground plane. The engine air induction or other air inlets should be included in such tests so that recovery and inlet flow distribution can be

measured. A facility of this type should be either combined with or placed adjacent to a wind tunnel where propeller performance can also be measured in forward flight.

3) No satisfactory substitute has yet been found for knowing the pull of the propeller on its shaft in flight, in order to define the performance of the propeller in the environment of the airplane. When the need is apparent, the development time for an appropriate device is too long. As we learn more about the design of propellers for static thrust, certainly the refinement in knowledge which the pull on the shaft would provide becomes more essential.

4) Convincing tests need to be performed to determine the minimum scale or actual sized propeller that will yield reliable and readily predictable performance for the full scale propeller. Besides enhancing research, it is suggested that the propeller manufacturer could then be better prepared to perform performance tests of such a propeller in the facility previously suggested at an early date consistent with initial wind tunnel tests of the airframe, either independently or in collaboration with the airframe manufacturer, to provide convincing proof that the expected propeller performance will be achieved.

Decelerating or Descending Flight at Low Speeds

In the early days of the XC-142 program, many people believed that wing stall in decelerating or descending flight at the transition speeds between hover and fully airborne flight was the Achilles heel of the tilt

wing design. We were fully confident that this was not the case. First, the VZ-2 had shown significant improvements in transition flight capability with rather modest leading and trailing edge devices. Concurrently, NASA wind tunnel tests indicated that proper high lift devices on a wing fully immersed in the propeller slipstream should provide the margins required. In addition, and perhaps most important, we had performed our own wind tunnel tests which indicated that descending transitions could be performed with state-of-the-art high lift devices and through special attention to the direction of propeller rotation. These early tests showed that for powers equivalent to descending flight, surface flow would remain attached to the wing for descent angles in excess of 5° at any speed. The major assumption made in evaluating these tests was that absence of disturbed flow, particularly on the surfaces within the propeller slipstream, was probably assurance of buffet-free controllable flight at that condition. Conversely, the presence of separated flow was assumed to indicate descent limitation of unknown severity. The obvious concern with the assumptions was conservatism, since we know that our more conventional aircraft may at times have substantial areas of flow separation without encountering limiting flight characteristics. Since no better criteria existed for evaluating wind tunnel tests for descent limitations, the airplane was assumed to be buffet-free when no separation was in evidence in the model slipstream; to have some form of buffet of increasing magnitude as surface flow separation on the model became evident and increased in severity; and that as force data reached a conventional maximum lift, controlled descent was no longer possible. At the very low speeds where the airplane is primarily thrust supported, descent angle limits are quite high

by these criteria since no maximum wing aerodynamic lift in the normal sense is identifiable in the force data at required thrust levels.

With no substantiating flight experience to give us a quantitative guide as to the airplane's descent capability, a substantial amount of wind tunnel testing was devoted to this one characteristic, consisting of tailoring wing center section stall prevention devices, leading edge flaps and slats, and the configuration of the trailing edge flaps to insure the maximum possible capability within the general restraints of the configuration. We believe that LTV was the first to demonstrate the importance of the direction of propeller rotation on descent boundary, as documented in reference 4. More recently, confirming tests on the direction of propeller rotation have been run with a two propeller configuration and are discussed in reference 5. Further encouragement was received from the NASA free flight model tests of the XC-142A which indicated a good descent capability based upon flying qualities, and gradual degradation of flight behavior as a warning of ultimate limitation. Descent capabilities as indicated by all the related tests of the XC-142 are indicated in figures 3 and 4, and are compared with comparable flight data. Figure 3 compares buffet onset with predictions, and figure 4 compares maximum usable descent capability with prediction. Except for the Ames 0.60 scale model tests, model tests under-predicted the airplane capability, although buffet onset appears to have been closely predicted by our own tests. In addition, it is to be noted that significant margin exists between buffet onset and maximum usable descent rate on the airplane. Obviously, we are gratified that the airplane results are better than the wind tunnel predictions if they must be different. In addition, we can state

that so far the actual descent capability of the airplane appears to be very adequate, although it must be appreciated that many tests remain to be performed. Eventually, the evaluations of the XC-142 will undoubtedly lead to a better definition of desirable or even required descent capabilities and it will be essential that we know how to design this precise capability into an airplane with a high degree of confidence. Today, however, we are uncertain as to the absolute contribution of each high lift device to descent capability and are therefore unable to identify conservatism in the design if it exists.

All of the scale model tests were correctly interpreted to indicate a descent capability but the differences are considerable and are attributed for the most part to scale or Reynolds number. In the past, this kind of problem has been resolved by first identifying the detailed aerodynamic characteristics and properties that are different in the full scale tests and then devising model tests or corrections which reliably predict or duplicate the full scale results. Obviously, the more emperical the corrections become, the more data are required on a variety of configurations to gain confidence in these correction factors or special tests. While it is very encouraging to see the NASA and our competitors continuing to devise improved configurations for slow flight of the tilt wing airplane as evidenced by reference 5, we believe that it is essential to perform all possible correlations with flight tests as soon as possible in order to increase our basic confidence in wind tunnel testing for prediction of descent capability.

Specifically, we recommend and hope that both the XC-142 and CL-84 airplanes will be made available for specific descent boundary correlation

tests at some point in their respective programs. The XC-142 makes an excellent vehicle for this purpose because of the variety of configurations possible, i.e., the possibility of inboard or outboard slats extended or retracted independently, a flap position variable through a large angular range for a fixed wing position, the effects of center section ramp removal, etc.

The need for such correlation is brought to our doorstep in the study of the C-142 configuration, where the aerodynamics engineer is continually being asked if the slats and ramps can't be removed from the airplane in the interest of weight saving and simplicity since the descent boundaries of the airplane are greater than expected. As engineers with limited knowledge, we are reluctant to deviate far from what works until we know the full consequence of the change, and our conservative reply is test it on the airplane and if it works, we will all be happy. Unfortunately, in such cases, the airplanes are frequently involved in other programs obtaining answers to other problems of an urgent nature; hence, no change for the time being.

In addition to flight data correlation on descent boundaries, we urge continued research to improve configurations for maximum maneuvering capability at low speed such as described in reference 5. It is easy to show today that tilt wing technology consists of two or three very specific point designs whose missions in life are quite different and that the holes in information in between are very large. Therefore, it is very difficult to quantitatively predict the effect of change. Unfortunately, this may be a fact of life with the VTOL because of the large potential number of concepts

and the inability and even the inadvisability of documenting each configuration as has been done with the more conventional aircraft in the past. We are pleased, therefore, to see the NASA sponsoring programs such as the recent Short Haul Transport Study as a means of weeding out the non-competitive concepts. Perhaps a wider application of such programs is in order.

Flying Qualities in Hover and Low Speed Flight

The third area of primary concern from an aerodynamics standpoint was the stability and control of the vehicle at hover and transition speeds. Because of the requirements for flight under IFR conditions, attitude stabilization in pitch and roll was considered essential from the outset. Since we felt we were treading in somewhat virgin territory with this airplane, it took a while to convince ourselves and then to convince others that our problems were "simply"

a) that the dynamic behavior of the machine was not unusual for a hovering vehicle
b) that the machine had ample resources of control power
c) and that when hooked together properly, the controls in combination with the stability augmentation systems provided a very controllable airplane under IFR conditions.

Our analysis became a massive, complicated, iterative integration as we gained understanding and appreciation of our problems. When calculation was insufficient and confirmation was required, resort was made to a Flying Qualities Simulator, first with little aircraft hardware, and eventually with nearly all control system hardware. These simulators (which are described in

references 6 and 7), along with their analytical support, were the heart of our flying qualities investigations, and the success of the airplane in flight speaks well for the competence of this work.

As our work progressed, it became obvious to us that designing to IFR flight was conservative for the majority of flying that the airplane would be required to perform, and that the same control system would be over-designed to an unknown degree for VFR flight. To date, no flights have been made under IFR conditions so the control system has never been tested under the environment for which it was designed. In view of the generally universal acclaim for the flight behavior under VFR conditions, the dilemma for us today is whether the system is over-designed even for the IFR case. Already, we have eliminated the requirement for an altitude damper, but this is more likely due to pessimistic estimates of natural height damping. Yaw damping has been found to be a marginal requirement for VFR flight. Control power appears to be more than adequate about all axes based on VFR conditions encountered to date. These control parameters are the most easily measured of all the low speed aerodynamic derivatives and for the cases of yaw and roll control power are compared with estimates and XC-142A specification requirements in figure 5.

In summary, by hard work and good judgement, we have achieved a VTOL airplane with a reputation for very good flying qualities in the hover and transition flight modes. From a research standpoint, however, two very important questions remain unanswered for the XC-142 and therefore hinder the efficient development of other VTOL aircraft. The first question is simply,at what point by actual test in a real environment while performing

real tasks do the flying qualities of the airplane degenerate from good to no good as control power, sensitivity, damping, stability augmentation, control harmony, forces, etc. are varied. The second question is, are the simulator tests performed in the design of the airplane valid or conservative.

The principal recommendation to be made is that a concerted program be vigorously and consistently pursued to evaluate the many flying VTOL aircraft from a control requirement standpoint, and that this program include, of course, the XC-142. Surely, with the number of these aircraft now flying, both here and abroad, it should be possible to see tangible results in this important area in the not too distant future. However, we caution that for this program to achieve full return, planning ahead is required so that each vehicle receives a consistent evaluation. Secondly, such a program needs very careful correlation with simulator or analytical evaluations in order to better establish the validity of these tools for both design and research.

New Problems Encountered in Flight Tests

From an aerodynamic standpoint, the low speed flight characteristics relating to the V/STOL flight regimes have been very successful and proven not only the feasibility of the tilt wing concept but that the XC-142 itself is aerodynamically sound as a vertical or short take-off and landing airplane. With the improvement in static thrust in hand, there are no other low speed aerodynamic problems presently identified as detracting significantly from the operational capability of the airplane.

We believe that the problem encountered in flight which is of most interest from a research and development standpoint is the phenomenon of

unsteady airplane motion at very low airspeeds in the presence of the ground. Specifically, at combinations of wing incidences and flap deflections indicated in Figure 6, the airplane encounters random disturbances, principally in yaw, at altitudes from five to forty-five feet above the ground which are difficult to control because of the abruptness with which they occur. The speed range involved is approximately 12 to 30 knots. The severity of this problem was not expected. Although no specific investigations had been made to determine if such a problem existed, extensive wind tunnel tests had been made in the presence of a ground plane studying the effect of ground height upon maximum lift or descent capability without sensing indications of this problem. Since the VZ-3 was the only other aircraft with large flow turning capability which had made flights close to the ground at low speed, assistance from NASA Ames revealed well documented data showing that the VZ-3 had encountered severe disturbances near the ground at low speed which were attributed to recirculation of the propeller slipstream. This XC-142 problem has since been investigated further in the LTV wind tunnel, the NASA 17 foot tunnel at Langley, and in flight in order to gain a better fundamental understanding of the cause and possible alleviation. Reference 8 contains a summary of the Langley tests and a correlation with the flight data. The conclusion of the investigations so far is that the problem is a result of the airplane flying through its own turbulent slipstream under conditions where the combination of wing incidence, flap deflection, and forward speed produce a forward component of slipstream velocity greater than the speed of the airplane.

Fortunately for the airplane, the problem occurs at a range of speeds not proven to be critical to the operational suitability of the airplane. While STOL landing distances at heavy gross weights are adversely effected by inability to perform normal landings at the speeds where the disturbances occur, this disadvantage can be readily offset by greater use of reverse thrust and wheel braking. Optimum STOL take-offs are made at non-critical combinations of wing incidence and flap deflection. Ample speed is available for maneuvering in the hover configuration and satisfactory control and behavior have been demonstrated in high surface winds.

Investigation in the wind tunnel and subsequently in flight has shown that the magnitudes of the disturbances and the altitudes at which they are encountered can be reduced significantly by altering the flap programming such as to depend more directly on thrust and less directly on wing lift for support (i.e., reduced flap deflections) of the airplane. However, the speed range at which the disturbances are encountered has not been appreciably altered.

While wind tunnel investigations using smoke and tufts have been helpful in understanding this problem, it should be appreciated that all that the wind tunnel has provided to this point is knowledge that certain flow behavior exists around the airplane at the time that the airplane is having difficulty. In fact, we have reproduced this much on the airplane itself through smoke studies made in flight. Obviously from a research, development, and design standpoint, much more is needed. First we must improve our flow visualization techniques in the wind tunnel; then we must develop a prediction capability of detecting if disturbing forces accompany

flow phenomena, a means of measuring the magnitude of these forces, and means of predicting the controllability of the airplane when flying through these disturbances. We believe the development of techniques to accomplish these purposes to be most important to the orderly development of future VTOL aircraft since many of the configurations now being studied may be susceptible to similar or even more serious ground effect problems than those peculiar to the XC-142.

The most important aspects of this paper are believed to be summarized in the following remarks:

a) It is heartening to see the analytical programs underway to improve the predictability of propeller static thrust; it is recommended that a parallel program be undertaken to provide improved facilities for measurement of the actual capabilities of propulsive devices, including propellers. Recognition is made of the important contribution being made through the propeller static thrust facility at Texas A&M under the sponsorship of NASA. However, we visualize a facility of broader capability sufficient to handle a variety of propulsion devices.

b) The tilt wing concept is well established as a feasible, practical, and important configuration in the V/STOL field; now it is necessary to perform the correlations with flight test and to continue tests in the wind tunnel so as to determine the full potential of the tilt wing and how to provide the most for the least.

c) If the development of VTOL airplane handling qualities is to avoid the long development history of present day airplane handling requirements, more advantage needs to be taken of the knowledge that can be gained from the VTOL airplanes currently flying. In the VTOL airplane, the handling qualities must be right but not one (1) pound more than right. Confidence in our analytical and simulation techniques will be enhanced by such tests.

d) With the number of V/STOL propulsion system concepts on the market today, we cannot expect research agencies to provide generalized configuration data to support each concept to the extent that the conventional airplane has received in the past. More than ever, it would appear that the inventor of each new concept must provide or obtain his own funding to establish basic feasibility and the "place in the sun" the vehicle deserves before expecting support from other agencies. In addition, comparative studies such as the NASA Short Haul Transport Study may help reduce the variety of vehicles upon which research should be expended. In the meantime, agencies such as NASA must always be on the lookout for basic research which could serve a wide variety of interests in the VTOL field.

e) The experiences of the XC-142 in ground effect at low speed should serve as warning to other V/STOL vehicles and point to the specific need for means of predicting the existence and severity of such problems very early in the design cycle.

Many tests are currently being performed to study the recirculation problems of the jet VTOL through flow visualization wit' the emphasis on thrust and moments. Unsteady forces may exist for some of these configurations and are undetected for lack of proper techniques. For some configurations, we suggest that the Princeton Track Facility may possibly provide means of detecting the existence of a problem, and some insight into its severity. The free-flight model technique which NASA Langley has used very effectively for other purposes might be extended to this problem.

f) It is obvious in many instances that the research suggested requires pilot evaluation and detailed measurement of flight characteristics of an airplane. Government procurement contracts generally provide for such tests only to the extent necessary to show that the airplane is capable of satisfactorily performing a mission. In a gross sense, then, it is only when problems are encountered that the engineer has license to investigate and determine in detail how the performance of a system has compared with predictions. Consequently, as aerodynamics engineers, it appears to us much of the time as though our learning curve on the flying airplane (compared with our detailed predictions) is directly proportional to the number of problems the flying airplane encounters. Obviously, we don't want the problems, but we do want the correlations which are vital to continued improvement in design. Since airplanes and

instrumentation are expensive, however, research funds are rarely available to support the necessary flights of the airplane. In addition, competitive pricing and the fixed price contract means that the flight program will deviate less than ever from only the most essential of tests. We suggest that the only real solution to this problem is to establish the requirement and payment for such tests as part of the basic procurement of the airplane with the tests to be performed after the more urgent demonstrations are completed. To some degree, it can be argued that the extensive structural flight load measurement tests required by the Air Force, and the more recent requirement for flight measurement of engine thrust by the Navy are precedents for such tests. Just as reasonable programs are devised for major demonstrations, it should be possible to devise reasonable programs which will yield stability and control derivatives and drag parameters for quantitative comparison with prediction.

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STATIC THRUST IN HOVER

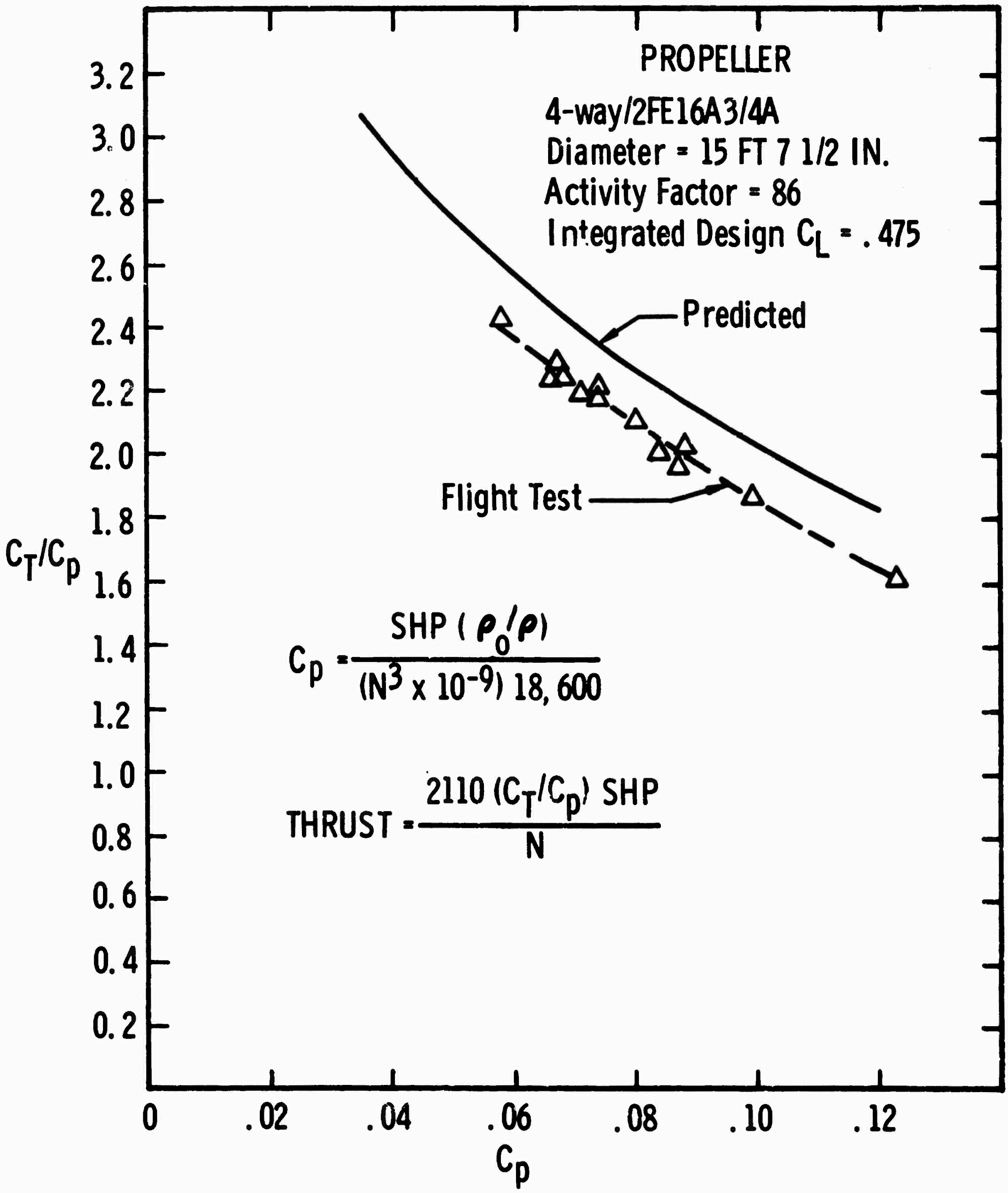

Figure 1

PROPELLER STATIC PERFORMANCE

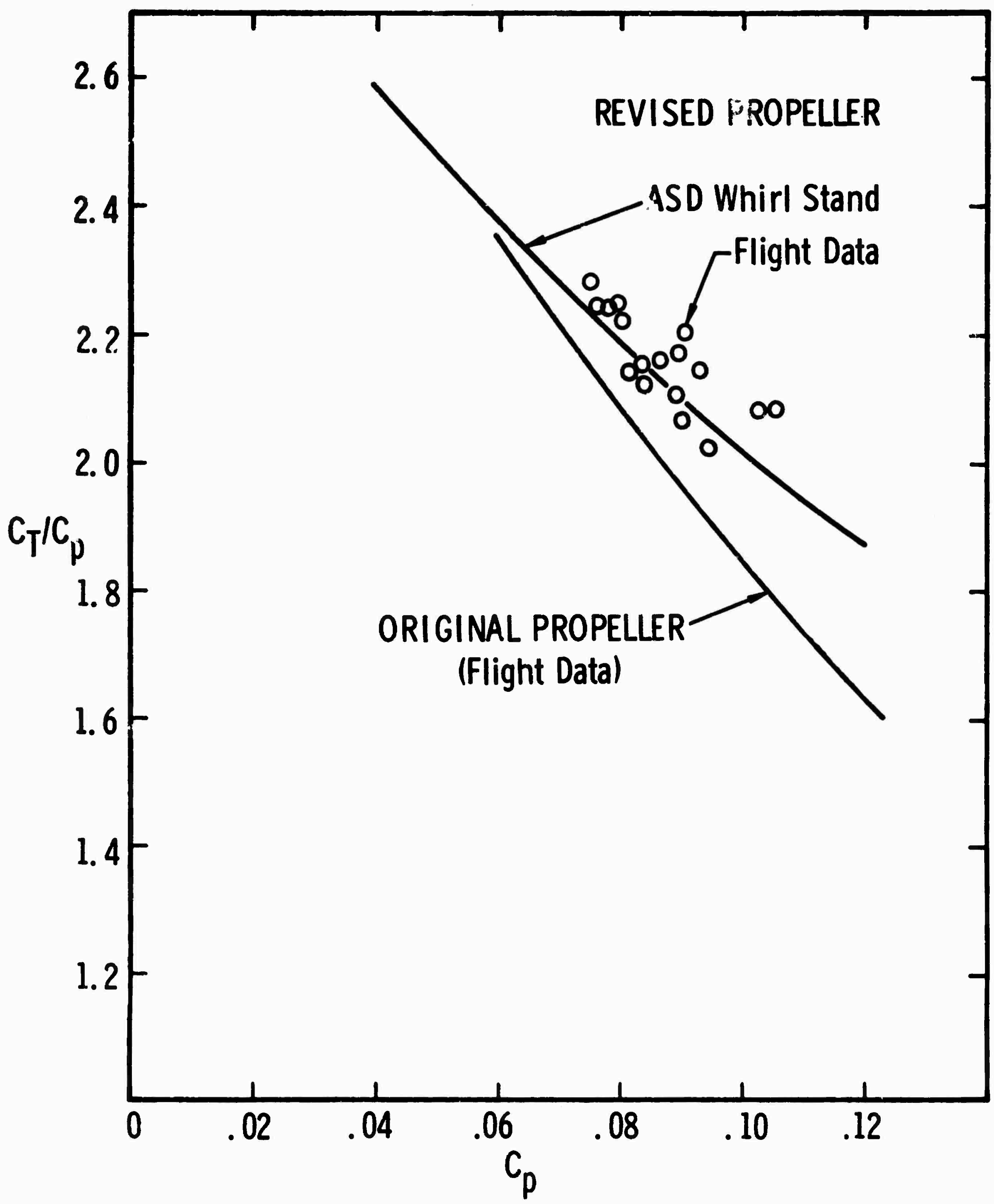

Figure 2

BUFFET ON-SET DESCENT BOUNDARY

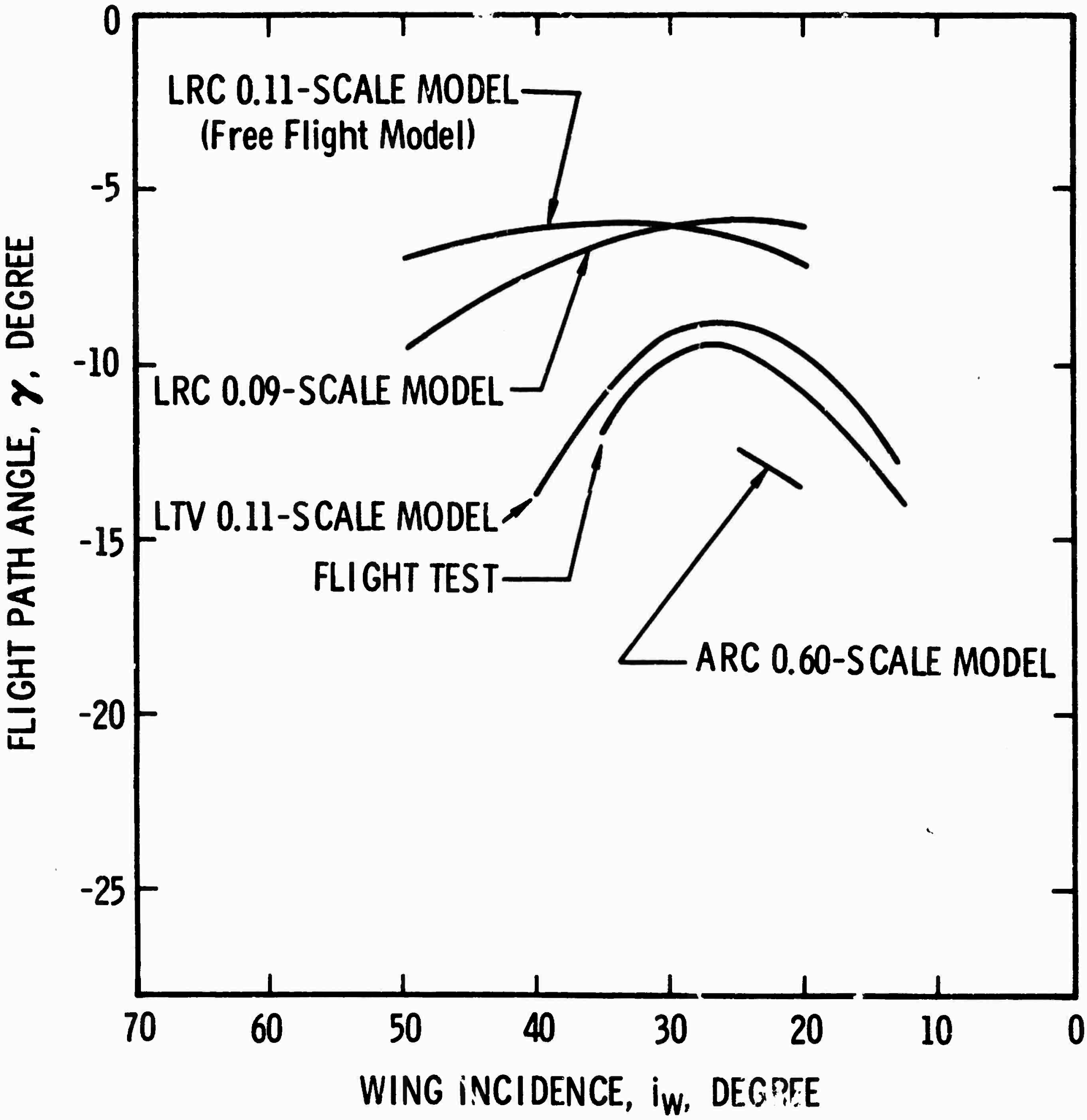

Figure 3

MAXIMUM USABLE DESCENT BOUNDARY

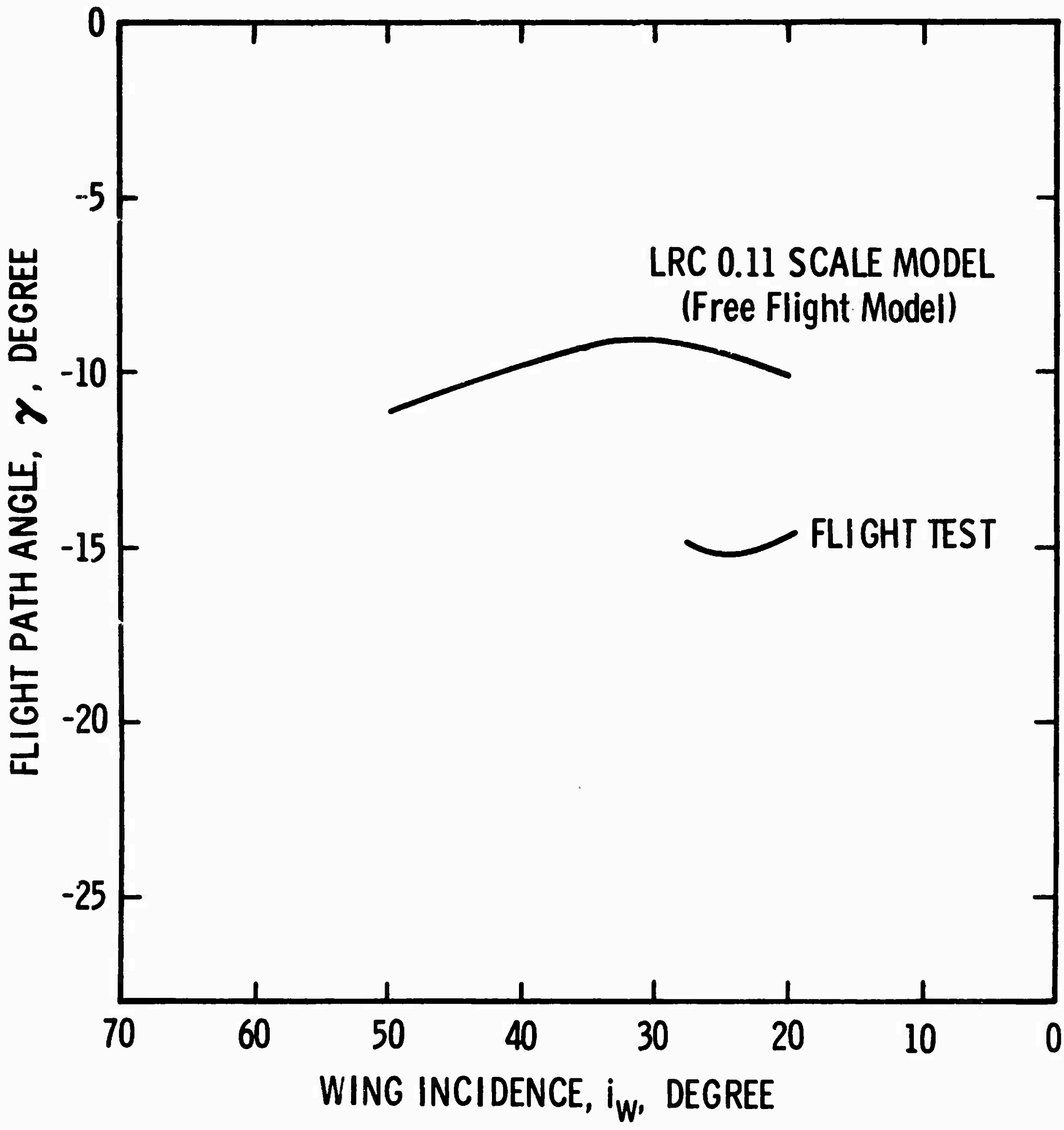

Figure 4

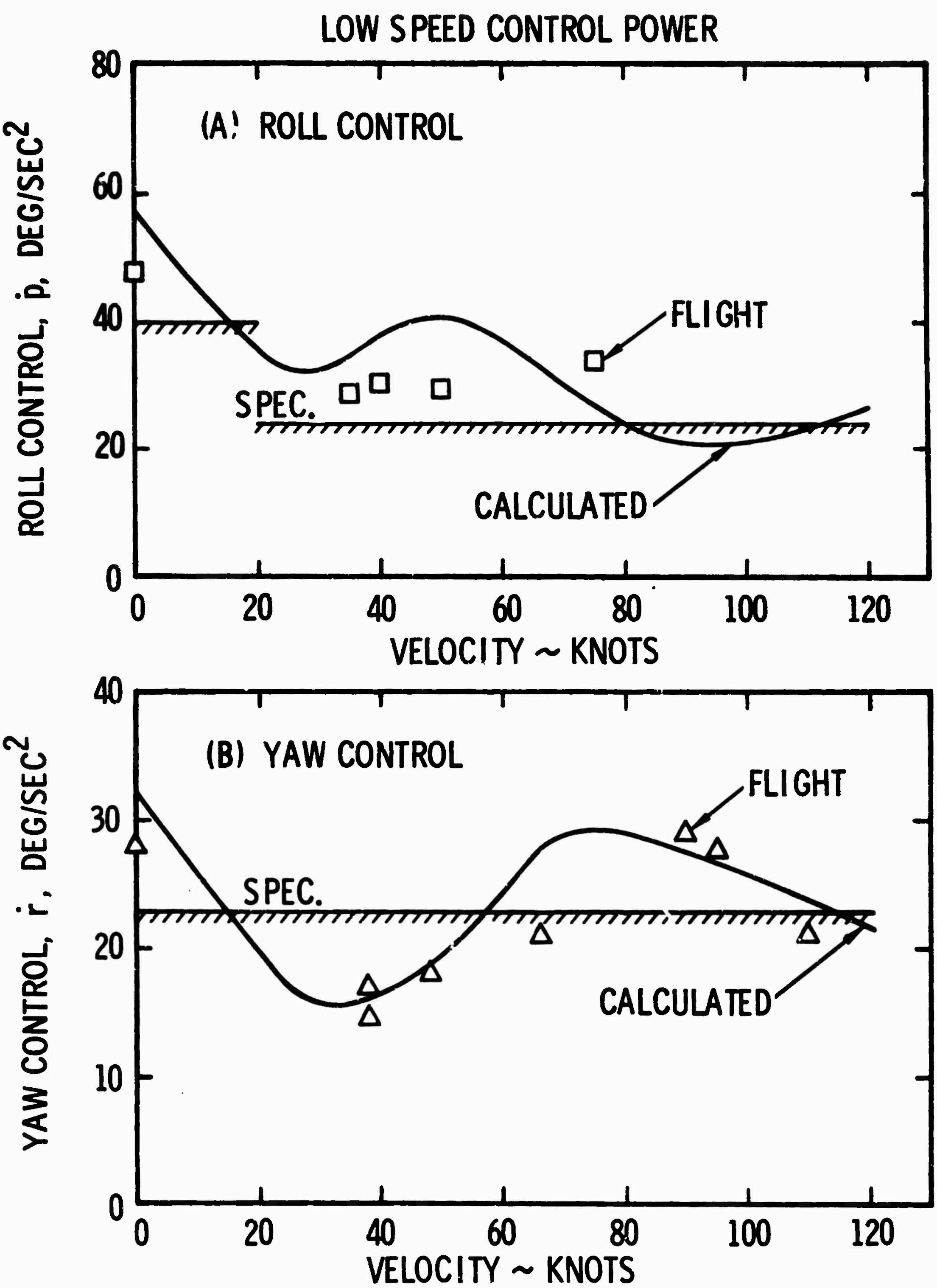
LOW SPEED CONTROL POWER
(A) ROLL CONTROL
ROLL CONTROL, ṗ, DEG/SEC²
80
60
40
20
0
FLIGHT
SPEC.
CALCULATED
0
20
40
60
80
100
120
VELOCITY ~ KNOTS
(B) YAW CONTROL
YAW CONTROL, ṙ, DEG/SEC²
40
30
20
10
0
FLIGHT
SPEC.
CALCULATED
0
20
40
60
80
100
120
VELOCITY ~ KNOTS

Figure 5

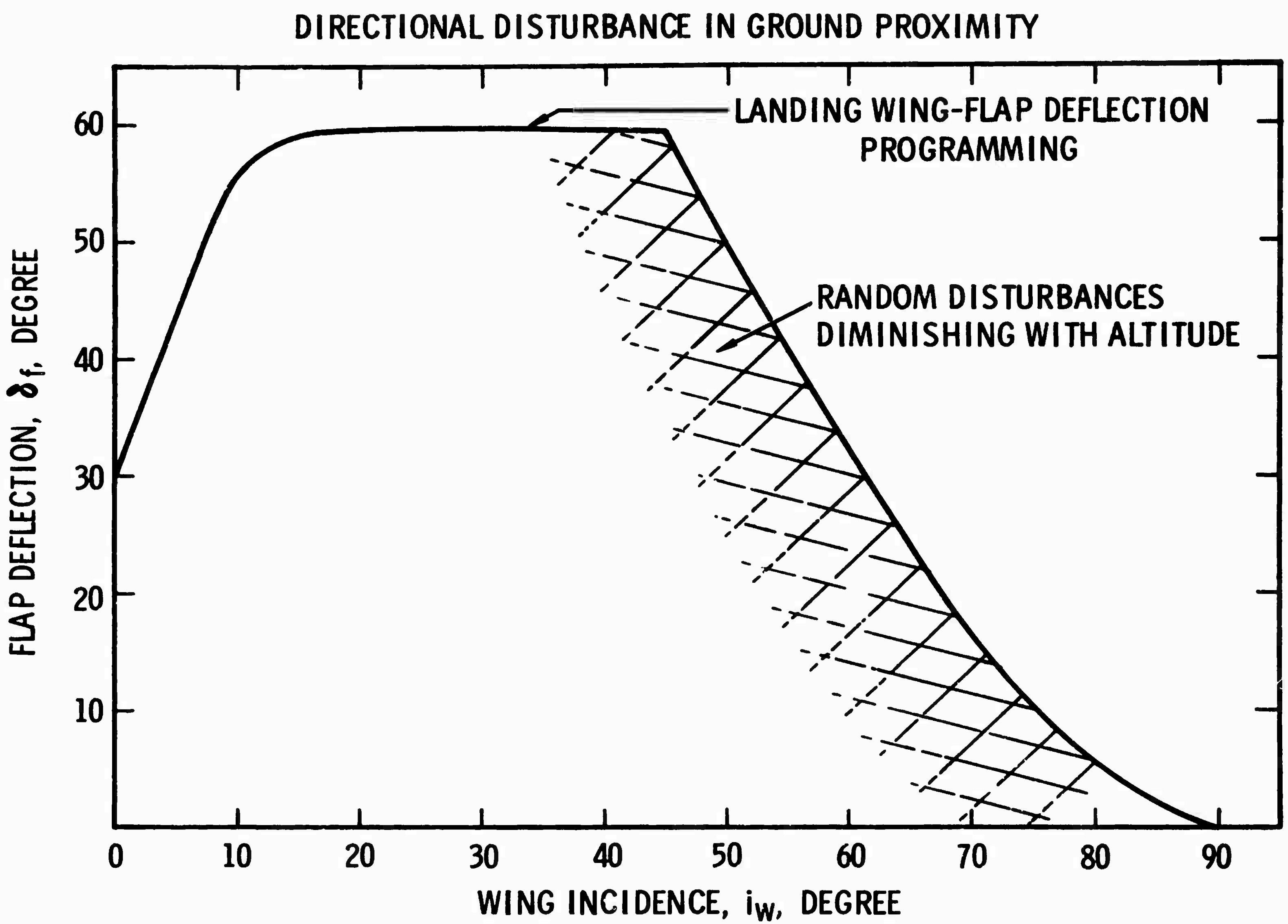
DIRECTIONAL DISTURBANCE IN GROUND PROXIMITY
LANDING WING-FLAP DEFLECTION PROGRAMMING
RANDOM DISTURBANCES DIMINISHING WITH ALTITUDE
FLAP DEFLECTION, δ_f, DEGREE
60
50
40
30
20
10
0
10
20
30
40
50
60
70
80
90
WING INCIDENCE, i_W, DEGREE

Figure 6

PANEL I

QUESTION AND ANSWER PERIOD

CHAIRMAN HEWIN: I think it has been occasionally mentioned that you should state your name and affiliation when asking a question. In this case, we would appreciate it also, if you wish to address your question to some specific member of the Panel, that you name to whom the question is addressed.

I would like to open by giving the Panel an opportunity to cross-question the Panel, if they desire. Do any of the Panel members have questions for the other Panel members?

MR. UPTON: I have a question I would like to ask Doug Henshaw. He mentioned that they had developed some techniques for extraction of derivatives in flight. I wonder if this applies to very low air speeds such as [are] encountered in STOL airplane flight?

MR. HENSHAW: My answer to that would be that the requirement is that we be dealing with the linearized equations of motion. If you feel that at these low speeds, for the particular vehicle that you have in mind, a linearized description is valid, then, our method will apply.

In fact, the method might conceivably take account of some nonlinearities; but, if the vehicle becomes nonlinear, I think the problem very rapidly becomes much more difficult, and it could easily happen that our method would become ineffective.

I might mention that, in our method, we basically use the well-known equations of motion method in which you measure the various motion variables — such as the bank angle, the rate of change of bank angle, the size of bank angle, etc. — and you substitute these measured values into the differential equations and you end up with linear algebraic equations in which the unknowns are the stability and control derivatives.

We applied the well-known least squares method [in which] we do claim one refinement which we think is significant. That is, we assume that all [of] the measured quantities are subject to a constant error during the measurement period. We put this in explicitly in the analysis. This sounds like a trivial addition to the analysis, I know, but we feel that, quantitatively, this is a very significant perturbation on a well-known technique.

MR. UPTON: The reason for my asking the question might be elaborated on a little bit. We have had extreme difficulty on the XC-142 in measuring aerodynamic derivatives — say at speeds below 50 knots. Other than those, it might apply to control power, the things that can exert a significant acceleration to the airplane. We have found that it is very hard to extract the damping derivatives from our data — that they tend to get lost in the noise levels. They represent very low forces acting on the airplane [when] compared to the overall inertia of the airplane under these conditions. So, this is a problem area also that is not mentioned in my words nor my paper, but it is a problem area for us in the future if we hope to determine how well we predict our derivatives in these very low speed areas.

MR. HENSHAW: I might add that our measurements have been made at speeds down to about 45 knots with a fixed-wing airplane. If you thought it was worthwhile, we might be able to show some of our results. I wonder if we might have slides 8, 9, and 12 [shown again]?

CHAIRMAN HEWIN: I think on the grounds that it may take them a minute to locate those, and since we are short of time, we might open up to a question in the meantime from the floor or from the Panel.

QUESTION: [F.N.] Piasecki, Piasecki Aircraft Corporation.

As is his reputation, Mr. Gustafson has made a beautiful and decisive thrust in the future, and I want to congratulate him.

First of all, on his very last point, I am all for the pooling of computer machine programs and, from our little company's standpoint, our Univac is ready, able, and willing.

Second, going backwards, on your point, Gus, as to a common theory or one theory, I am all for it, but I think NASA's job is to dictate what that theory is.

MR. GUSTAFSON: We provided an easily used theory but not with the intention that [it] be the sole thing used in any investigation — just that this be a common comparison . . . we have put one out. It was a contract situation.

MR. PIASECKI: And, as would be the rules-of-thumb, whatever you meant by that, I would agree that that, too, should be led by an agency such as yours.

On the more technical comments and details that you presented today, I am in full accordance . . . with perhaps a desire to expand it a little bit away from the rotor that you seem to be putting all of your comments on. All of your comments surround, I believe, a question of a rotating blade in a lift and propulsive situation.

MR. GUSTAFSON: In order to limit myself to ten minutes, I covered helicopters only . . . rotary wing only.

MR. PIASECKI: And, I would like to say that, in addition to those things, a consideration of rotating wing in combination [with] other lift and propulsive systems would be highly desirable to add to your program.

In certain cases such aspects may be in conflict. For instance, your statement as to the thinner airfoils and less camber at the leading edges and the tips of rotor blades, and the tip vortices control etc. for the lift control at the tip of rotor blades, may have certain changes when it's used in an unloaded condition such as a compound helicopter.

My last comment is [directed] to Mr. Henderson regarding [his] compound elevon and the triplane configuration. Your curves parallel very much

our experience, and, as a matter of fact, in our ring-tail duct propeller that we use in our compound helicopter, we have a triplane rudder, with each of them being compounded.

MR. HENDERSON:

That particular configuration is an alternate arrangement that we are going to evaluate on the X-22 airplane for hover and transition yaw control.

CHAIRMAN HEWIN:

[Referring to previous question by Mr. Upton and the answer by Mr. Henshaw which was delayed:]

Do you have the slides?

MR. HENSHAW:

This [referring to slide] is an example of one of our measurements. We have measured here the lag in the dihedral effect. This $\dot{\theta}$ is the rate change of sideslip angle, and the vertical bars come from the application of least squares. The data is such that 95 percent of the readings would be expected to fall within those vertical bars. So, it gives you some idea of whether [or not] the uncertainty in the measurement is large enough to justify including such a derivative in the analysis. I think we would admit that $C_{l_{\dot{\beta}}}$ is very small on this fixed wing Otter airplane we used in this experiment, and one would probably not consider using it in the experiment.

Can we have the next slide, please? This is a yawing moment lag term which is, once again, with respect to the rate of change of sideslip angle. We think that this effect is quite large. We see that for the power off, gliding flight, this term vanishes and, then, at high positive thrust, this term becomes larger. This term is comparable to the damping due to yawing rate, so that one should include this sort of thing in the analysis.

Once again we can get some idea of the scatter in the experiment from the interval toward the 95 percent confidence level.

Can we have the next slide, please?

This is just to indicate that the $C_{n_{\dot{\beta}}}$ term was comparable to the C_{n_r} term. These are generally typical of the type of measurements we were able to make with an aerodynamically simple STOL, fixed wing airplane. Thank you.

CHAIRMAN HEWIN: Do we have any questions from the floor?

QUESTION: [Richard K.] Koegler, Cornell Aeronautical Laboratory, Inc.

I have a question for Mr. Henshaw. The development of the indicial response technique seems to be a very useful one at this point, where we are going into more nonlinearities.

I have a question which I guess takes a bit of explanation to start with. I guess we spend most of our lives wrestling nonlinear problems into linear equations, when we come right down to it.

We start out by taking situations where, at one flight condition, we can find the slope of a curve, and, if we don't go too far away from that flight condition, we get some pretty good equations, and we can use [them]. If we want to change flight conditions, we can take the step-wise approach. When we start getting into time lags, we run into some other problems. Some of them are taking real and imaginary terms and putting part of them into a $\dot{W}$ term, or something like that, where, as long as the frequency involved in the motions or the time lags is long (relative to the solution time of this imaginary part of the equation), linear equations work pretty well.

Two things seem to happen, though. We have things like downlash slag from the wing to the tail, and you put this into a $\dot{W}$ term, and every once in awhile some poor soul puts a step input of W into that equation, and he gets some really good answers because he gets an infinite force for a moment.

Another thing that I know we encountered years ago in some tests of a PT-26 well beyond stall and which is of a similar nature — and it might have some bearing on Mr. Upton's question — when you are trying to reconstruct the events using equations of motion, the rebuildup of lift — the circulation reattachment at these angles well beyond stall — seems to take about a second at angles of attack of maybe 10 degrees beyond stall. So, you have some pretty slow effects once in awhile.

What I am wondering . . . my specific question then is — it would seem as though, for many of our uses and for everyone's benefit, that whatever ground rules etc. [that] Mr. Henshaw and his people are developing to indicate when you can and when you can't use these methods and how you get around some of the things like [when] you put a pulse input on but, like that $Z\Delta$ term [sic], it really doesn't build up instantaneously, but it's a little thing and it goes away pretty quickly anyway, so you don't really care if your equation showed that it built up instantaneously . . . but, some lift term that gave you an infinite peak of lift force would be sort of a nasty thing to cope with; so, you would throw out part of the expression after you had computed it — and I just wondered if he has any comments on techniques and ground rules and so forth [that] they have developed to usefully apply these concepts to various cases?

MR. HENSHAW: Yes, that is a very interesting point. In the brief time for the presentation, I didn't point out some of the problem areas. If you put in a lag term as indicated, you do get some pretty unrealistic outputs.

I am afraid our work in this area is fairly preliminary. We haven't thought deeply about this point at this time, but it was an indication to us in our flight test evaluation of derivatives that we should, in fact, use reasonably smooth control inputs during the maneuver so that we would, in fact, be doing a maneuver which was reasonably described by the mathematics. So, in this sense, we recognize this difficulty, but we haven't faced up at the moment to what we would actually do with this in some of the more detailed analyses.

I think with the impulsive admittance we would really not get this infinity, but [would] just get an instantaneous value in the quantity of time, $t = 0$. I think this might tax our intuition as to what to do with this.

CHAIRMAN HEWIN: Thank you. I am not sure [that] we are going to have time for any more questions. Dick [White], I presume you don't want us to alter the schedule.

Well, then, we will have to bring it to a close. I would certainly like to thank the Panelists for all of the work and thought that they have put into giving us some seductive suggestions.

Since I will probably be the last fellow from AVLABS up here with the microphone in his hand, I would like to take this opportunity to express my personal appreciation, and that of the whole AVLABS staff, to Dick White and all of the Cornell staff who have worked so hard on this Symposium to make it what I think has been a real successful one. I would like to suggest that we give Dick and his gang a real round of applause, as well as the members [of Panel I].

(Applause.)

SUMMARY

PANEL I

AREAS OF RESEARCH IN V/STOL AERODYNAMICS

L. M. HEWIN
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The detailed recommendations for research by the members of Panel I are described in the prepared papers on pages 167 through 290. The summary of these recommendations, as presented below in tabulated form, includes basic aerodynamics, propulsion, and wind tunnel testing. No attempt has been made to assign priorities to the recommendations in this summary.

1. BASIC AERODYNAMICS

- Determine, for full scale propellers, the boundaries of the vortex ring state as a function of forward speed, rate of descent, and disk loading
- Reduce unwanted effects of blade-tip vortices
- Study aeropropulsive interference effects
- Develop practical means of delaying stall on rotor blades
- Investigate aerodynamic characteristics of various types of surfaces in a propeller slipstream
- Develop a better understanding of the aerodynamic characteristics of 2- and 3-dimensional "high lift wings"

2. PROPULSION

- Investigate methods of reducing and predicting propeller and rotor download losses due to wings and bodies operating in the wake
- Test propellers at high shaft angles to provide data for the prediction of attendant aerodynamic forces and moments at the hub. In addition, more work is needed to define the nonuniform skewed wakes produced by propellers

- Provide improved facilities for measurement of the actual capabilities of propulsive devices
- Define autorotative characters of propellers with low disk loading in the presence of lifting wings
- Investigate the interaction of internal flows with external flows as it affects new types of propulsion systems
- Generate general information on the thrust, drag, and moment of isolated ducted propellers. In addition, investigate the effects of flow disturbances caused by bodies, surfaces, and slipstream interaction

3. WIND TUNNEL TESTING

- Determine reasons for differences in model and full scale results, particularly in the range between hover and conventional flight
- Test several propeller-wing combinations over a range of operating speeds to determine variation of lift and power due to translational speed
- Improve flow visualization techniques during wind tunnel testing
- Improve the prediction of wind tunnel wall corrections for powered V/STOL models

Technical Session VI

Panel II

Chairman

HAROLD A. CHEILEK, Vice-President - Technical

Cornell Aeronautical Laboratory, Inc.

Member	Affiliation	Prepared Paper - Pages
A. C. Adler	Hughes Tool Company	303 to 310
J.M. Drees	Bell Helicopter Company	311 to 321
E.A. Fradenburgh	Sikorsky Aircraft, Division of United Aircraft Corp.	323 to 338
G.H. Fries	Vertol Division, The Boeing Company	339 to 346
N.B. Gorenberg	Lockheed-California, Co.	347 to 355
L. F. Crabtree*	Ministry of Aviation, Royal Aircraft Establishment	357 to 360

* Mr. Crabtree was unable to attend the Symposium; however, his prepared paper is included in these Proceedings.

AREAS OF FRUITFUL RESEARCH AND DEVELOPMENT
FOR ROTARY WING AIRCRAFT

by

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I. SUMMARY

Three areas of publicly-funded research are suggested for continued improvement of helicopter performance capabilities. These areas are, to some degree, being explored by private corporations, but systematic government-funded programs would be of greater value to the entire industry. The three areas of particular concern are:

A. Determination of airfoil characteristics for rotor operation

B. Reduction of rotor hub-pylon drag by reduction of local interference and flow control

C. Summarization of current variable inflow research and preparation of working tables, charts, and card decks that could be used by industry for engineering applications in the immediate future

II. AIRFOIL CHARACTERISTICS

A. Operating Environment

Virtually all rotor theory developed to date, including the elaborate variable inflow and aeroelastic work, has been based on the use of two-dimensional airfoil characteristics. This fundamental approximation is open to question particularly in forward flight. The flow past any blade element is neither steady nor two-dimensional. Serious work on the various phenomena, such as centrifugal pumping and spanwise flow have only recently been under-

taken. Tanner and Yaggy have in their recent paper raised some questions with respect to the significance of centrifugal pumping. The effects of spanwise flow due to forward velocity have yet to be investigated experimentally.

It should be noted that the yaw angle experienced by upwind and downwind blade is on the order of 20 degrees at current helicopter maximum speeds and this figure rises to 25 degrees at the projected 200 miles per hour level. The yaw angles experienced in compound operation could be on the order of 50 degrees. At either level, this could hardly be called two-dimensional flow.

B. Airfoil Data and Rotor Performance Limits

The need for more exact knowledge of airfoil characteristics has been accentuated by the emphasis on increased helicopter speed. Current record helicopter speeds of approximately 200 mph appear to have become the projected requirements for the next generation of helicopters. The record speeds were reached with lightly loaded rotors operating at very low blade lift coefficients. This mode of operation is not conducive to either aerodynamic or structural efficiency. Overall improvements in efficiency could be achieved relative to the record machines by development of airfoils that could delay advancing blade drag divergence with no deterioration in retreating blade stall. Such airfoils would permit the use of slightly higher tip speeds and lower blade solidity. Reduction of the drag rise would improve performance; reduction of blade solidity would reduce blade weight.

To date, there has been no systematic test program ever conducted to determine rotor airfoil characteristics in forward flight. NASA has conducted a series of hovering tests with large rotors built with different airfoils. Attempts were made to theoretically synthesize airfoil characteristics from the performance data gathered in these tests. These data have been of interest for performance predictions since they generally indicate less severe limitations due to drag divergence and stall than would be estimated by use of two-dimensional data. Drag divergence appears to be delayed by an increment of .05 Mach number. The implied $C_{L_{max}}$ is generally somewhat higher than that of equivalent two-dimensional

data. The Army high-speed helicopter test programs have also indicated some relief with respect to drag divergence. The use of purely two-dimensional airfoil data has in several reported instances, given conservative estimates of power required in high speed flight regime. There are, however, no really firm definitive data or methods for translating from two-dimensional airfoil data to performance limits in high speed flight. One alternative is to base all performance estimates on two-dimensional data with the rationalization that these estimates are conservative and allow some margin for error. The other approach is to use synthesized inferred characteristics from the available sources of test data. This latter approach limits us to the use of airfoils that have been tested on reasonably large rotors.

In addition to the problem of correlating two-dimensional and actual rotor performance, there also remains the problem of the quality of the two-dimensional data. The data of greatest interest is at the extremes of the test range, $C_{L_{max}}$ at low speed, low positive and negative angles of attack at high Mach number. Since it is difficult to obtain accurate data at these extremes, there are likely to be differences in two-dimensional data for a given airfoil - let alone different airfoils. Performance advantages based on two-dimensional data run in different wind tunnels have to be considered with caution. Such claims contain possible differences in two-dimensional and actual rotor characteristics. Flight or large-scale model tests again appear to be needed to establish advantages for different airfoils with any degree of confidence.

The dilemma can be illustrated by consideration of this one figure which compares 0015 characteristics with those of a 10% "droop snoot" section. It should be noted that the data was obtained in the same wind tunnel. The $C_{L_{max}}$ achieved with the "droop snoot" is essentially identical with that of the 0015. The upper right-hand section of the figure indicates a gain in drag divergence Mach number of approximately .09. However, the calibrations between 0015 two-dimensional characteristics and NASA whirl tower data for the 0015 and measured OH-6 (with the 0015) indicate that the rotor drag divergence is delayed by

approximately a .05 Mach number increment relative to the two-dimensional characteristics. The problem...Would we achieve a .04 or .09 improvement with a "droop snoot" rotor?

In concluding this part of the discussion, it should be emphasized that we would not like to accept any deterioration in $C_{L_{max}}$ to achieve an improvement in the compressibility limits. Rotors have been flown and tested with 0006 and 0009 tip sections; however, no data has yet been published to indicate the effects on the retreating quadrant stall.

C. Test Facilities

Currently, no Government or publicly-supported wind tunnels remain for systematic two-dimensional airfoil testing. Several of the large, affluent companies in the rotary wing field do have facilities that can be adapted for these purposes. It would seem doubtful, however, that one of these facilities could be made available for a long-term publicly-sponsored program. Since the two-dimensional data is not really what is wanted, a systematic program of large-scale rotor testing suggests itself. The direct measurement of airfoil characteristics on an operating rotor will require a formidable development of instrumentation and test techniques. The direct determination of lift and possibly pitching moment and the synthesization of drag would raise the problems of test techniques and computational techniques. Such techniques will, however, have to be developed to determine the detailed, perhaps macroscopic, mechanics of rotor operation. The need for airfoil data is another reason for undertaking such a program now.

D. Recommendations

1. Review

Summarize results of all previous work relative to the determination of airfoil characteristics for rotors. Attention should be given to correlations between two-dimensional data and data from rotors. A review and summary of instrumentation problems should also be prepared.

2. Wind Tunnel Performance Testing

Extension of existing rotor performance limits should be undertaken in the immediate future. Program costs might be minimized by using modifications of existing small helicopter rotors and drive systems. The OH-6 could undoubtedly be made available as a test vehicle.

3. <u>Detail Airfoil Testing</u>

Upon completion of the "review" phase, a program should be formulated for systematic testing of airfoils and rotors. The models might take the form of large, stiff-bladed rotors which would simplify some of the interaction due to aeroelastic effects. By this time, it may also be possible to consider lift, drag and pitching moment measurements in the blade spars as well as measurement of lift loads by pressure distributions.

III. ROTOR HUB INTERFERENCE DRAG

A. Introduction

Sporadic wind tunnel programs have been performed by NASA and private industry to investigate means of reducing rotor hub drag and hub-pylon interference drag. Private industry has also conducted some investigation of drag control by local boundary layer control, but results have not been published publicly. In view of the ever increasing interest in high speed performance, a continuing systematic experimental effort appears to be warranted.

B. Recommendations

1. <u>Reduction of Interference</u>

A wind tunnel program exploring the effects of the listed geometric variables should be performed. No attempt should be made to restrict the investigation to current configurations. The objective should be to determine if any significant drag reduction can be achieved within practical geometrical constraints. The fundamental geometric variables are:

a. Hub height with respect to fuselage
b. Pylon height with respect to hub and fuselage
c. Pylon thickness ratio
d. Fore and aft location of hub with respect to pylon
e. Fore and aft location of pylon and hub with respect to fuselage zones of extreme curvature.

2. Flow Control of Drag

Wind tunnel investigations should be made to determine the mechanism of external flow control by "ramps" or "horse collars" and the feasibility of using blowing or suction for drag reduction. If the internal flow methods are promising and the flows required are small, means of using the basic gas turbine suction or blowing should be explored.

IV. ENGINEERING USE OF VARIABLE INFLOW THEORY

A. Introduction

Some of us not engaged in the current research in variable inflow have watched recent developments with curiosity, awe, and some reservations. This recent period of research has resulted in a number of different theories, inflow models, and comparisons with rather recently obtained rotor blade load data. To date, no clear-cut firm justification or recommendation has been made for use of variable inflow theory in normal engineering practice. If the theory has been developed to the degree that it can be used to some benefit for engineering estimates of performance, loads, and vibration levels, a commonly acceptable method (to the researchers) should be presented to the industry now. There is no intent, in this suggestion, to stifle further research in this field. The objective is to make use of the specialized knowledge we already have at hand. Further refinements could be included as they are developed.

B. Recommendations

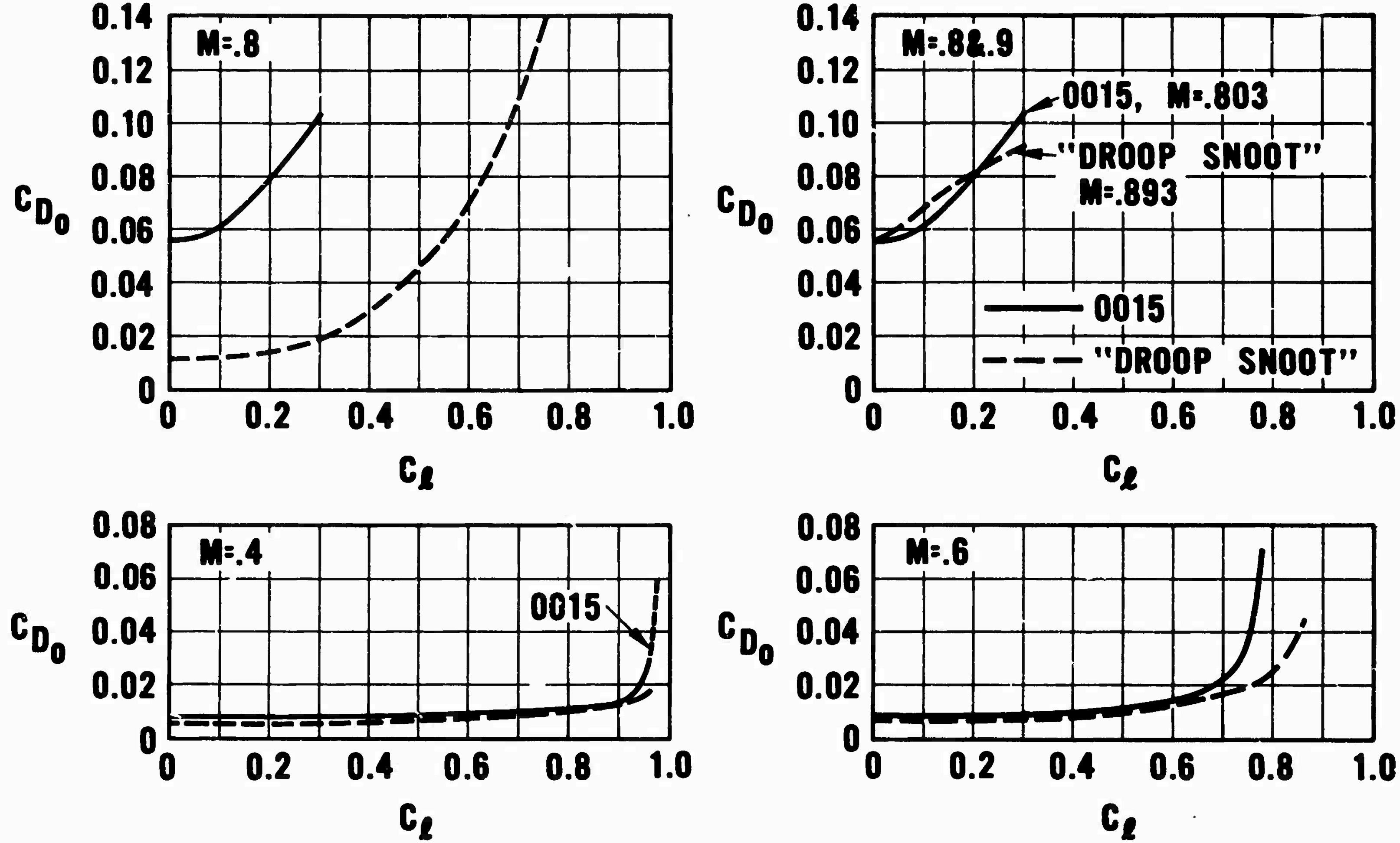
1. Summary of Current Research

The effects of variable inflow estimation of rotor power requirements, rotor operating limits (blade stall and Mach number) as well as on blade loads should be established. The portions of the flight spectrum and the technical areas wherein the variable inflow theory can be used profitably should be defined from these analyses.

2. Engineering Methods for Computing Variable Inflow

The development of a standard general method for dealing with the variable inflow should be undertaken. Consideration should be given to the least complex procedure that appears to give adequate accuracy. An attempt should be made to define the inflow in terms of the basic parameters that are used for the stiff-bladed blade element theory. Hopefully, the final output might be in the form of charts or tables that define the variation of local inflow ratio with radius and azimuth angle with collective pitch, average inflow ratio, thrust coefficient, power coefficient, and tip speed ratio as parameters. This form of presentation would permit machine storage for the more complex problems and still permit limited manually performed investigations that are of interest in day-to-day engineering work.

COMPARISON OF 0015 AND 10% "DROOP SNOOT" SECTION
DATA FROM AMES 1'x3.5' WINDTUNNEL



A COMEBACK OF LOW-SPEED AERODYNAMICS RESEARCH

by

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INTRODUCTION

This aerodynamic symposium could, in my opinion, be considered as a sign of a revival of interest in low-speed aerodynamics by research institutes, governmental agencies, and industry. This renewed interest in aerodynamics can be clearly observed in helicopter development.

As a typical illustration of aerodynamic evolution, Figure 1 shows the development that took place at Bell Helicopter Company during the last 25 years. It is seen that immediately after the helicopter proved its feasibility, great attention was devoted to aerodynamic finesse. Note the wheelcovers on the second Model 30, and the streamlined tailboom. Soon, however, it was discovered that streamlining did not pay off, but actually incurred an unacceptable reduction of payload. At this point, a long period started in which there was little or no respect for aerodynamics. Only recently, with cruising speeds of the new designs raised well above the 100-knot mark by the introduction of powerful, lightweight turbines, has the helicopter aerodynamicist been re-established in his proper function. This is evidenced in Figure 1 by the Model 206A with streamlined skid gear (retractable gear does not pay off yet), inlet design, etc. Similar developments can easily be traced in other helicopter companies.

In rotor aerodynamics, one may notice a parallel development. Profile selection deviated little from the traditional

NACA 0012 profile. An attempt by NACA to introduce low-drag helicopter profiles in 1954 (Reference 1) was not pursued, despite indications of appreciable improvements in hovering efficiency. Only during the last few years has attention to refinements of rotor aerodynamics been noticeable: tapered thin blade tips were introduced (Reference 2), new profiles are being developed (References 3 and 4), and attention is being devoted to blade root aerodynamics (Reference 5).

In a broader sense, the same trend can be observed. This is illustrated in Figure 2. This figure shows the results of an approximate count of NASA, AIAA, and AHS reports and papers on aerodynamic subjects produced per year on low-speed, supersonic, transonic, and rotary-wing aerodynamics. The shift in attention toward supersonic and transonic aerodynamics in the years 1945 to 1955 is obvious. After 1955, a sharp drop in total aerodynamics activity is noticeable. A transfer to space research may have caused this. Also noted during this period is a closing of many wind tunnels. Rotary-wing aerodynamic research kept increasing until 1957, but seems to have dropped off gradually since that time. Lately, however, this downward trend is believed to be reversed. Some of the wind tunnels are being reopened. Airfoils are again being tested with combinations of Reynolds numbers and Mach numbers in the range of interest to helicopter aerodynamicists.

The task of this panel is to recommend areas of research to pursue in this time of increased attention to the aerodynamic possibilities. Instead of listing the obvious areas that should receive attention, such as blade tips, rotor hubs, fuselage, wing-rotor interferences, etc., I will try to approach the question in a slightly different way.

RESEARCH APPROACHES

It is an open secret that in every organization, almost without exception, confusion exists about what to classify under research and what under development. Of course, they overlap in many ways. More often than not, new ideas are created and fundamental answers are found during hardware-development programs. This is understandable in the light of the pressing need to bring particular hardware projects to successful conclusions.

This type of close-to-the-product research is most useful and profitable. It is, however, not possible even to try to predict this type of activity; it is not necessary either. It will follow its own cause, directed by the needs of the moment.

Another type of research activity comes when we stand back a little from the immediate day-to-day problems and try to anticipate the needs in the near and far future. Many studies of this kind can be classified as configuration research. Work on new concepts such as compound helicopters, trail rotors, stopped rotors, etc., fall in this category. Also, wind-tunnel rotor tests, flight tests in the 200- to 300-knot speed range at high advance ratios and high advancing-tip Mach numbers, and many others are included in this research. A wealth of new aerodynamic information is available or forthcoming from these programs which are being sponsored by the government and industry. Well known are the many high-performance helicopter programs, wind-tunnel tests in the full-scale wind tunnel at Ames, and Freon-tunnel test programs at Langley.

It is noted that research on rotor configurations in wind tunnels differs from fixed-wing wind-tunnel research. Scaling of rotor models, aerodynamically and dynamically, is a great problem, especially if blade torsion and control-system deflections are also being considered. Testing in the wind tunnel of full-scale

rotors, however, is at best still limited to 200 knots in the NASA-Ames 40- by 80-foot wind tunnel. This is very unfortunate because major problem areas can be foreseen in rotor operation in the 200- to 350-knot range, where advance ratio and advancing-tip Mach number approach unity simultaneously. Testing of rotors in this speed range must therefore take place without the benefit of full-scale wind-tunnel testing. Consideration should be given to creating facilities for full-scale testing of rotors at forward velocities well above 200 knots.

Aerodynamic research of a more fundamental nature, of course, should have been mentioned to begin with. It is the type of work that everybody feels should be done in the first place. It is amazing, however, to realize how little has been done in this direction during the past decade. The remainder of these comments are therefore devoted to this matter of fundamental aerodynamic research and promotion of this type of activity.

EXAMPLES OF INSUFFICIENT BASIC UNDERSTANDING

From many indications, it seems that the insight in many a fundamental low-speed aerodynamic phenomenon is far from complete. I personally got a mild shock when recent tests at Bell proved my ideas about boundary layer flow to be wrong. Tanner reported on these studies during this symposium and in Reference 3. During the same investigations, it became apparent that the induced flow in the case of an optimum hovering rotor may not be a uniform velocity distribution. Furthermore, the origin of the tip loss factor is in question: is it an aspect ratio effect or, more likely, a loss due to high skin-friction drag caused by very high velocities from the tip vortex?

Equally intriguing is the practical feasibility of laminar-flow profiles. NASA rotor-tower tests proved years ago

that the 8H12 profile produced performance advances (Reference 1). For various reasons though, many aerodynamicists remained quite skeptical about the practical applications. More often than not one hears comments that laminar flow will not work because of:

(a) turbulence of the flow through the rotor,
(b) leading edge erosion,
(c) fabrication problems, or
(d) skewed flow in forward flight.

Such statements, however, must be proven. Recent Bell investigations showed no effect of the turbulence of rotor flow, a considerable tolerance to a pitted leading edge, and contemporary blade fabrication methods to be adequate for laminar flow profiles. A laminar-flow blade in skewed flow has, as far as is known, never been tried. The given example serves to show that we must be more readily inclined to analyze these matters, conduct experiments, develop measuring techniques, and, last but not least, test the principles on full-scale hardware throughout the operational speed range.

To continue this list of unanswered questions, for years tests by NASA and industry have shown compressibility effects to be overestimated on the basis of two-dimensional airfoil characteristics (References 6 and 7). Where are our efforts to explain this by theoretical calculations and by special wind-tunnel experiments? Similarly, stall on rotor blades seems to show mysterious characteristics. Many rotor aerodynamicists maintain that blade stall is delayed from a rotor-lift standpoint but may still be counted on for profile drag increase (Reference 8). Bell's rotating hot wire tests show stalled flow to exist. Of course, in forward flight the unsteady flow field complicates matters, and basic research is known to be underway in this direction.

No attempt is made to present a complete picture, only to illustrate the embarrassing lack of basic knowledge on key issues. It is refreshing to see that in this symposium some of these subjects are discussed in the papers presented. Before concluding these comments, I want to bring forward one more aspect of aerodynamic research that, in my opinion, should receive more attention.

IMAGINATIVE RESEARCH

The majority of research presently being performed follows the predictable path: a problem exists, we try to solve it; a vacuum in our knowledge exists, and we explore that area. Some research, however, follows a less predictable trend. Once in a while, stimulating new lines of thought are opened. To borrow two examples from other fields:

a. Hydrodynamicists found the as-yet unexplained fact that certain chemicals, added to water, will retard transition from laminar to turbulent flow and thus reduce skin friction drag (Reference 9).

b. The biologists are wondering why certain types of aquatic animals (salmon, porpoise) swim much more efficiently than one can explain with the known laws of nature (References 10 and 11).

The type of research that may result from the questions that these studies reveal may prove to be very fruitful when transplanted to the rotary-wing field. It is important to realize in this respect that the Reynolds numbers at which rotor blades operate (below 3×10^7) are quite low. This opens possibilities not available to fixed-wing aircraft, which often operate at Reynolds numbers above 3×10^7 where it becomes increasingly difficult to achieve laminar flow. Figure 3 illustrates this.

This kind of basic and imaginative research is often seen as a free, unrestrained activity. I do not believe this is the case. The researcher should have a very specific goal in mind. Of course, the outcome of this work may surprise him in that it may be much more practical for uses other than he originally envisioned. The cost of this sort of research is usually quite moderate. The timing is hard to schedule. It requires inquisitive minds and a comprehensive education, in addition to a fair amount of perseverance to overcome the usual skepticism of contemporaries.

CONCLUDING REMARKS

There are indications that low-speed aerodynamics is receiving renewed attention. The most pressing need in future research is to fill the many gaps in our understanding of a great number of basic aerodynamic principles. In addition, there is room for a more vigorous and imaginative search for new approaches. In certain areas, it still seems as though we are just beginning.

Of course, parallel to these fundamental research activities, it is necessary that the R&D work related to detailed refinements, new configurations, and the exploration of new regimes of flight will take a major share of our attention. Concern is expressed that full-scale wind-tunnel testing of rotors is presently limited to 200 knots, while the low-disc-loading VTOL's are ready to explore the 200- to 300-knot range.

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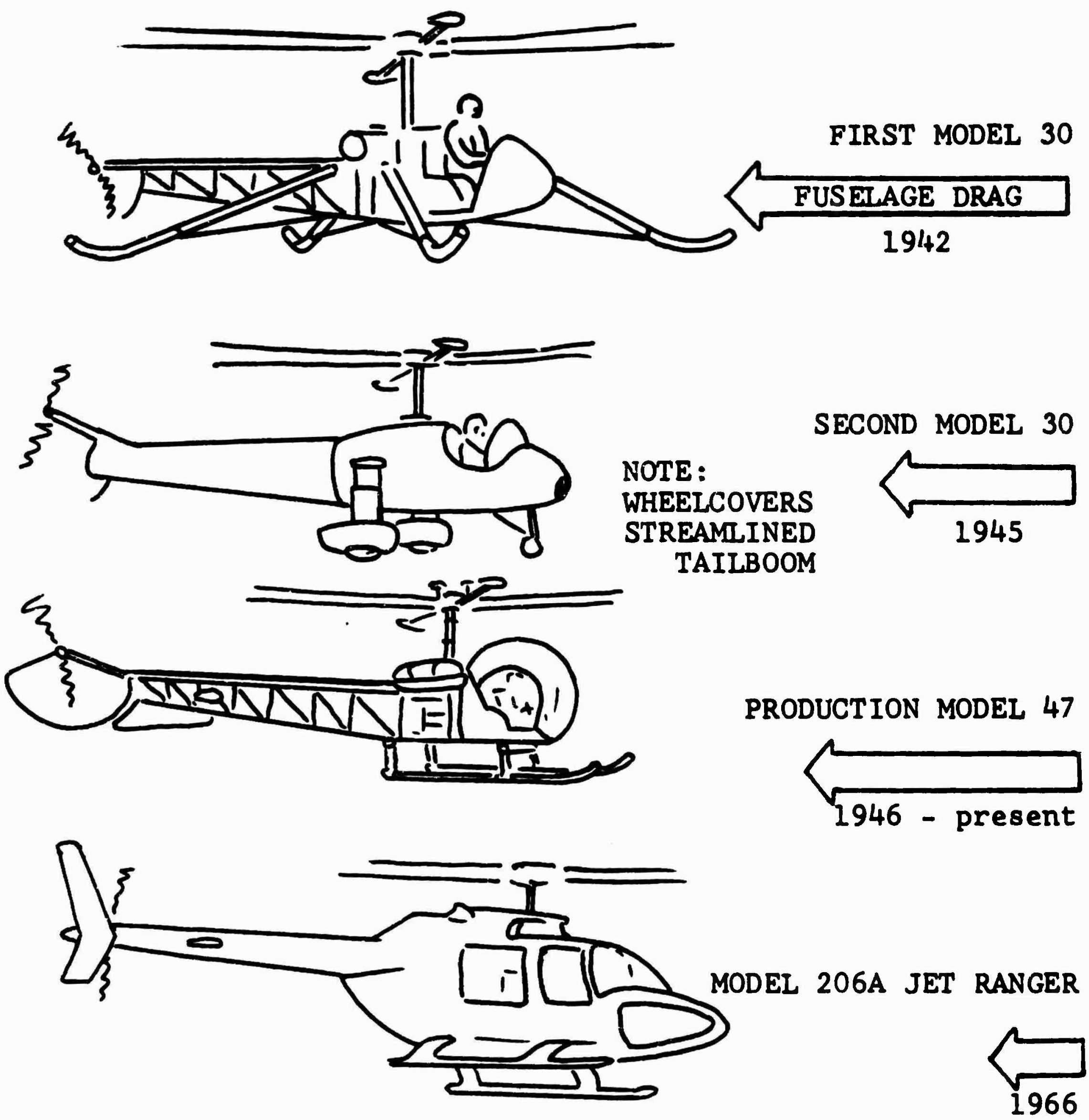

FIGURE 1. EXAMPLE OF ATTENTION TO AERODYNAMIC DETAIL IN PERIOD FROM 1942 TILL PRESENT

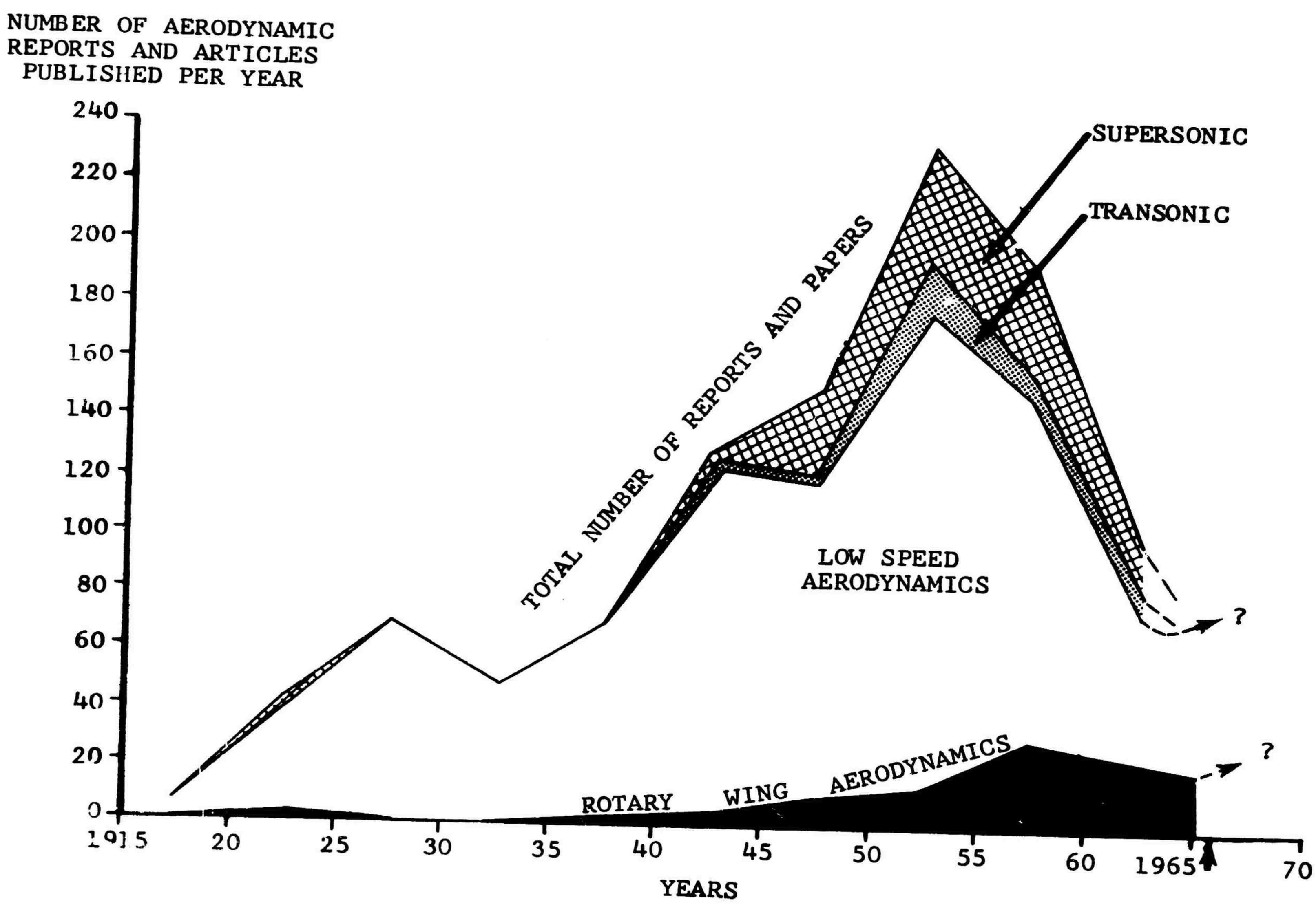

FIGURE 2. HISTORY OF AERODYNAMIC RESEARCH ACTIVITY

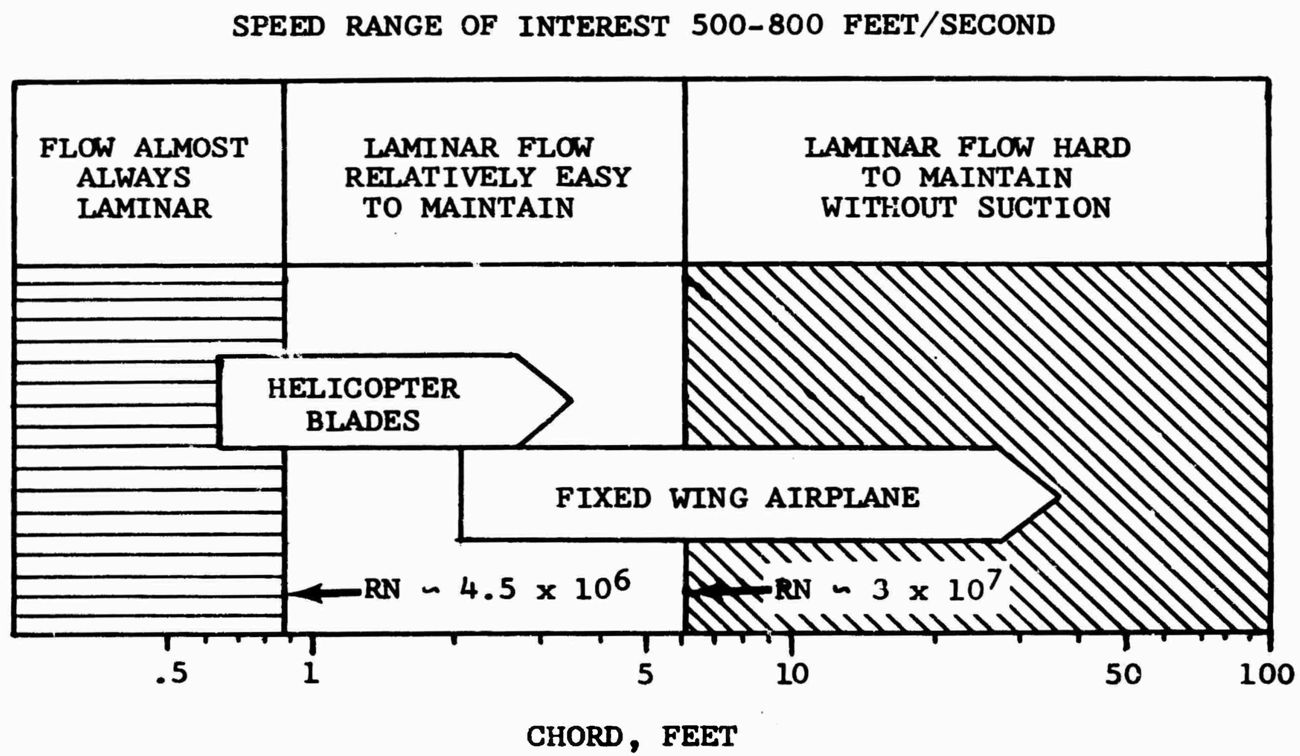

FIGURE 3. AERODYNAMIC IMPROVEMENTS EASIER TO ACHIEVE FOR ROTOR BLADES THAN FOR FIXED WINGS

REQUIRED AERODYNAMIC RESEARCH FOR V/STOL AIRCRAFT

by

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Assistant Chief of Advanced Research

INTRODUCTION

Although the helicopter is still the only vertical takeoff and landing aircraft in operational use, many other types have been developed to the research aircraft or flying prototype stage. Soon both commercial and military users will have a much broader selection of aircraft in the vertical and short takeoff and landing categories from which to choose. This latter statement assumes that various problems associated with performance, flying qualities, and operational suitability factors of the newer types of rotor and non-rotor V/STOL aircraft will be solved, because, for any configuration to survive as a permanent type, it must be possible to demonstrate reliable mission accomplishment at an economically justifiable cost. With the proliferation of new V/STOL configurations in the last decade have come an even greater number of unknown or unexplained aerodynamic characteristics. While many of these have received considerable research attention, there is a great deal more to be done before the V/STOL designer can predict the overall aerodynamic characteristics of a new aircraft with any degree of confidence.

Sikorsky Aircraft, with perhaps more total helicopter experience than any other manufacturer, has conducted a substantial amount of research on non-rotor V/STOL aircraft as well as on numerous advanced rotary wing types. This effort has resulted in a keen appreciation, however, that no single organization of any type can fulfill all of the research needs of the industry. Government conducted and supported research programs and public dissemination and exchange of research information, as typified by this Cornell/Avlabs Symposium, is vitally important to the future success of V/STOL aircraft and is therefore heartily endorsed.

Some of Sikorsky Aircraft's research programs and design studies have identified areas requiring further aerodynamic research as outlined in the following section. It should be noted that certain technical disciplines which are closely related to aerodynamics, particularly flying qualities and aeroelastic phenomena, have been largely excluded from the list as it is understood that basic aerodynamic flows are of primary interest in this panel discussion.

RECOMMENDATIONS FOR AERODYNAMIC RESEARCH

Airfoil Section Data

Additional experimental data on airfoil section characteristics are needed, especially high subsonic Mach number characteristics of airfoils suitable for helicopter rotor blades. These data are needed for predictions of both performance and rotor dynamic behaviour.

As an illustration of how airfoil section characteristics can affect rotor dynamics, an unusal phenomenon recently encountered in flight tests of the Sikorsky S-61F research compound helicopter (Reference 1) will be described briefly. It was observed that whenever the local Mach number at the tip of the advancing blade exceeded a level of about 0.92, an apparent "split" in the tip path plane occurred, visible to the pilots as two lines formed by the blade tips forward of the nose of the aircraft instead of the usual single distinct line. The magnitude of the "split" increased with increasing Mach number. Examination of experimental data combined with theoretical analysis has established with reasonable certainty that the observed behaviour is an aeroelastic response of the blade to highly non-linear airfoil pitching moment characteristics at high Mach numbers. The airfoil in this case is an NACA 0012, which has a critical Mach number at zero lift of approximately 0.725, so that at 0.9 or above the characteristics are dominated by supersonic flows and shock patterns. Unfortunately, there is no thoroughly reliable section data for this airfoil in the transonic regime. Figure 1 illustrates in a qualitative manner what can be expected, however, based on other existing two-dimensional airfoil data. Note that, at the high Mach number, an unstable region (positive slope) exists at low angles of attack. This unstable moment, combined with very high local dynamic pressure and a torsionally flexible blade, results in significant dynamic twisting of the blade with a further resultant change in overall blade loading. For the particular case in question, the dynamic response characteristics of the blade, which involved flapwise bending as well as torsion, were such that for one revolution the blade had a small positive angle of attack at the

advancing tip, twisted rapidly to a higher blade pitch condition, and followed a high tip path over the nose, whereas for the next revolution it encountered a small negative angle at the same azimuth position, twisted in the opposite direction, and followed a lower path. This is illustrated in Figure 2, which shows calculated time histories of blade twist, tip angle of attack and tip motion (flapping plus bending) for an extreme case. Note that the cycle repeats every second revolution, so that the motion is properly described as a one-half per revolution subharmonic. This motion explains the "split" in the tip path plane. The calculation was based on assumed section characteristics above Mach 0.9 and cannot be expected to provide more than a qualitative understanding of the problem, although agreement with experiment was quite good. The assumed nonlinear moments were necessary in the calculation; when they were removed the blade dynamics immediately returned to the normal cyclic behaviour wherein all motions repeat each revolution.

It is also of interest to note that another Sikorsky helicopter, the Marines CH-53A, has flown with advancing tip Mach numbers greater than 1.0, without encountering the same phenomenon. Both the airfoil section and the blade natural frequencies are somewhat different than those of the S-61F, and it is not certain which of these physical differences causes the difference in dynamic behaviour. It will be necessary to obtain reliable airfoil section data for a range of airfoil geometries before new rotor designs can be made with reasonably reliable predictions of rotor behaviour at high Mach number operating conditions.

Both two-dimensional wind tunnel tests and three dimensional tests of suitably instrumented rotor blades will provide valuable data. Examples of the latter approach are given in another paper presented at this symposium, Reference 2, which in fact show rather peculiar chordwise pressure distributions on the blade at high local Mach numbers, tending to confirm the unstable pitching moment characteristics discussed above.

In addition to steady state airfoil section data, more information is required under unsteady conditions over a wide range of Mach numbers, angle of attack, and time derivatives of both. Data obtained on an oscillating airfoil in a United Aircraft two-dimensional wind tunnel several years ago show sizable effects of particular importance to blade behaviour. Under certain conditions near blade stall, for example, negative damping values were measured under oscillating conditions, leading to the possibility of a local stall flutter over a portion of the rotor disk in certain forward flight conditions. More data are needed to permit adequate definition of this and similar problems.

Rotor Wake Geometry and Induced Velocities

The importance of this subject is well appreciated, and the general problem is probably receiving the proper level of research attention at the present time. This effort should definitel, be continued, as the current theories are not entirely adequate, as is pointed out in Reference 2. Specifically, the problems of rotor wake distortion (vortex-vortex interaction), unsteady aerodynamics, finite chord effects and transient effects in maneuvers need better definition. The general problem of predicting rotor and propeller flow fields continues to impose a limitation on the designer's ability to provide an optimum aircraft from the standpoint of performance, vibration, and structural loads. It should be mentioned, however, that it is most important to simplify the theoretical analysis of induced velocity distributions in conjunction with attempting to extend the scope of the analysis, for other wise the calculations will become too costly even on the most modern digital computers. Because flow visualization studies generally show a rapid roll-up of the vortex sheet, it is recommended that attempts should be made to replace the trailing sheet from one blade with two finite vortices - one from the tip and one from the inboard region of the blade. These vortices should not, however, be endowed with infinite velocities at the core.

While the forward flight conditions for rotors or the transition regime for tilt propellers are certainly the most complex, it should be mentioned that even the static hovering case needs considerably more definition. Theoretical methods for predicting hovering performance are frequently in error by several percent, occasionally resulting in a rather devastating effect on aircraft payload. A thorough understanding of wake contraction below the rotor or propeller, which strongly affects aerodynamic loading distribution at the blade tips, is required.

Aerodynamic Interference Studies

This is a broad and complex subject which has received considerable attention but which must get considerably more. VTOL aircraft generally utilize some multiplicity of lifting and propulsive devices, which affect each other in various ways in various regimes of flight. An example of this is shown in Figure 3, which presents information on lift interference of a rotor on a wing mounted directly below it. These data were obtained from a wind tunnel test of a compound helicopter model, some preliminary results of which appear in another paper presented at this symposium (Reference 3.). The graph shows the mean downwash angle at the wing per unit rotor lift as a function of forward speed, for a wing having a span of one-half of the rotor diameter, for three vertical positions of the wing on the fuselage. The data

points shown were determined by means of measured wing forces as influenced by rotor lift. The solid line is the theoretical mean induced velocity at the rotor disk due to rotor lift, and it may be seen to agree closely with the measured data for the low wing position, but that the high wing results appear to require a multiplying factor. These results are presented merely as typical of the type of information that a V/STOL designer must have available in order to guarantee a successful aircraft. There are many other interference effects that must be considered. For example, even restricting the problem to a wing below the rotor, the wing spanwise load distribution is affected, producing a wing rolling moment, and rotor lift is affected by wing lift level, as are blade flapping motions and blade vibratory stresses.

While the tests mentioned provide a substantial contribution to knowledge of interference effects on compound helicopter, much more remains to be done on both the compound and other VTOL configurations. Among the items needing study are rotor-propeller interference, drag interference studies (particularly with the objective of achieving a low drag rotor head design), and tail effectiveness studies. Gust response studies of compound helicopters are needed to determine rotor dynamic behaviour including interference effects, to permit development of rotor stabilization techniques at high advance ratio. Theoretical treatments of rotor-wing-propeller interference need to be developed (this of course is closely related to the subject of rotor wake geometry and induced velocities discussed in the preceeding section). For the tilt-wing-propeller VTOL, both theoretical and experimental data are needed to establish the division of steady loads between wing and propeller and the vibratory aerodynamic loading on both. While identical in principle to the problem of the compound helicopter, the tilt-wing propeller situation is more extreme because of the much higher propeller induced velocities involved and because of the problems of wing stall in transition, particularly under decelerating or descent conditions. In this connection the aerodynamic flows over wing leading edge slots and trailing edge flaps, which may be only partially submerged in the propeller slipstream, need better definition with a view towards optimizing the devices specifically for the VTOL application.

Vertical Drag in Hover

This is an additional aerodynamic interference effect, but is listed separately because of its importance. Vertical drag in hover can be as high as 5 percent of gross weight for a conventional helicopter, and for certain compound helicopters or tilt propeller VTOL configurations the penalty can be much higher. Unfortunately, this effect is not thoroughly understood and

predictions of vertical drag have occasionally been rather seriously in error. The situation is complicated to a certain extent by fuselage "buoyancy", which can result from the fact that the finite fuselage volume is immersed in a pressure gradient field which exists below the rotor, as well as by the fact that the rotor performance is also affected by obstacles in the downwash field. Finally, the influence of ground effect is very pronounced, being capable of changing the vertical drag into an upload, depending on the particular configuration involved.

An example of two of these factors is shown in Figure 4, which is reproduced from Reference 4. It presents the vertical drag measured on two simple disk models tested below a model helicopter rotor as a function of rotor height above the ground. The actual force on the disk is shown as a solid line; however this is not the effective vertical drag, because in the presence of the disk the rotor generates more thrust for a given power than it does without the disk. The lower dashed curve for each model represents the net vertical drag force, which was only about one-half of the actual downwash force measured on the disk. At the lowest heights above the ground ($Z/R = 0.5$) the actual and net vertical drag for each model was negative, that is, an upward force was induced by the proximity of the ground. This favorable effect was in addition to the normal benefit that the isolated rotor would receive from ground effect. Similar results have been obtained for other simple vertical drag models.

Because of the possible magnitude of vertical drag forces and the consequent influence on hovering efficiency, it is recommended that basic experimental and analytical research into the aerodynamics of vertical drag in hover, in and out of ground effect, be conducted for various generalized rotor and propeller VTOL configurations.

Ground Effects on Downwash and Aircraft Loads

The ability to predict the influence of the ground on aircraft performance, stability, and control in hover or low speeds (or in winds) depends on a knowledge of the slipstream patterns produced by the lift generators, whether rotors, propellers, fans, or jets. Because this downwash is generally not visible, it is sometimes difficult for the designer to remember that it is only the downward momentum of this air which supports the vehicle, and that when the ground or any other influence changes the flow pattern substantially, then the forces and moments on the vehicle are also likely to be substantially altered. Additional studies are required, therefore, to determine aircraft forces, moments, downwash patterns and interference flows between adjacent slipstreams when in the presence of the ground. Tests need to be conducted in various aircraft pitch and roll

attitudes through a range of translational velocities to permit determination of stability derivatives as well as performance as influenced by the ground. Such tests will allow a more general understanding regarding the relationship between downwash flows and aircraft forces and moments.

Slipstream Impingement Studies

Closely related to the previous item is a study of the aerodynamics of slipstream impingement on the ground. The objective here is establishment of criteria for acceptable downwash values for various missions. The downwash dynamic pressure is determined by disk loading, but the momentum of the downwash flow is determined by gross weight. Downwash irregularities caused by interference patterns between adjacent slipstreams are determined by aircraft geometry. Pickup of stones or other particles is determined by dynamic pressures, area of impingement, interference patterns, specific operating environment and the pilot's operational techniques. Consequently there is at present no reliable method of predicing the operational limitations due to downwash of a new V/STOL aircraft. As part of the effort required to establish the required criteria, much more theoretical and experimental work is required on particle pickup by the slipstream, on recirculation patterns surrounding the aircraft, including the effects of wind or aircraft translation, and particle trajectories through all parts of this recirculating downwash field.

Rotor Inplane Forces

As helicopter speeds continue to increase, the components of rotor resultant force parallel to the tip path plane become increasingly significant. In hover the net inplane force is zero, and at low to moderate speeds it can be neglected for most purposes, i.e., it may be reasonably assumed that the rotor resultant force is perpendicular to the tip path plane. At high speeds this is not so, but current rotor theories do not accurately predict the magnitude of the net inplane forces. Recently, for example, flight tests of a high cruise speed helicopter indicate that lateral rotor forces and moments differ significantly from predicted values. These differences show up statically in roll attitudes and lateral cyclic stick required for trim, and dynamically by the "Dutch Roll" characteristics being substantially different than present theory predicts. Rotor theory must be developed to improve correlation with experimental results. It is believed that proper treatment of the radial flow along the blade, due to the component of free stream velocity parallel to the blades, and possibly radial boundary layer flow, must be included in the extended rotor theory. These radial flow effects may also be expected to influence the rotor propulsive force capabilities at high forward speeds, and so are essential to rotor performance prediction.

Aerodynamics of Stopping Rotors

Of great future potential are V/STOL aircraft configurations which utilize a helicopter rotor for low speed flight and which then stop this rotor and operate as a conventional airplane in high speed flight. There are a number of options as to what to do with the rotor after stopping, including leaving it extended as a wing or stowing it away not unlike a retractable landing gear. No matter what happens after the rotor stops, however, the aircraft must at some point operate with the rotor turning at very low rpm, down to zero, so that the rotor advance ratio (ratio of forward to tip speeds) increases from normal values in helicopter flight up to infinity during the transition.

Sikorsky Aircraft has been conducting a series of experimental studies of rotors under stopped and slowly rotating conditions during which a number of pertinent phenomena have been made apparent. Some of the results of this study are shown in Figure 5, which presents rotor pitching and rolling moments for two different rotors under stopped conditions as a function of azimuth position of one of the blades of each rotor. The rotors had two and three blades, respectively, and had the same diameter and total blade area (solidity 0.1). Collective pitch was zero degrees, and shaft angle of attack was 5 degrees, corresponding to a reasonably mild vertical gust at conversion speed superimposed on an initial zero angle of attack, zero lift condition.

A somewhat unexpected result is that hub moments for the three bladed rotor are substantially larger than for the two bladed rotor. These differences can, however, be qualitatively explained by simple aerodynamic considerations. Referring to the sketches of the three bladed rotor drawn at certain azimuth positions, in the case of pitching moment at 60 degrees azimuth, the rotor has one blade pointing directly upwind and two blades which form a wing with a 30 degree aft sweep angle. Because of the extremely low aerodynamic aspect ratio of the blade pointing forward, the lift slope of this blade is very low compared to the other two so that, although the centroid of the blade area is at the center of rotation, the aerodynamic center of the rotor is well behind this point, resulting in a large stable moment (negative pitching moment at positive angle of attack). For a blade at 120 degrees azimuth, however, the situation is reversed, with the ineffective blade aft and the two effective blades forward so that a large unstable moment is observed. Considering the rolling moment, the maximum positive value was recorded with a blade at 30 degrees azimuth, at which condition there is one "straight wing" on the left side and two highly swept ones on the right. The combination of blades on the right suffers with respect to lift slope not only because of the sweep but also because the trailing vortex from the tip of the

forward blade will result in a significant downwash and loss of effectiveness of the trailing blade. Again, when the rotor is indexed an additional 60 degrees in azimuth, the geometry is reversed and negative rolling moment peak is observed.

The two bladed rotor, therefore, benefits from two factors relative to the three bladed system. First, the "sweep" angle of the two blades is always the same, and second, the tip vortex from the forward blade does not impinge directly on the aft blade except for the fore-and-aft position. The moments which were recorded on the two bladed rotor, which are still by no means negligible, are attributed to blade radial flow effects (sweep effects), the disturbance created by the rotor head for blades near the aft position, and the difference between airfoil characteristics under "leading edge first" and "trailing edge first" conditions.

These effects are all compounded in complexity when the rotor is turning. It has been found that even for a tip speed of only one-tenth of the forward speed, the characteristics of the rotor can be significantly different from those of the stopped condition. Many more investigations of this general nature are required in order to permit adequate design of stopped rotor aircraft. Analytical investigations of rotor behaviour at extreme advance ratios are needed, including the effects of finite span, radial flow, and a variable induced velocity field.

Ducted Flow Studies

For V/STOL configurations which utilize internally mounted lifting devices, such as fans or turbojets, a different class of aerodynamic problems arise than occur with unducted rotors or propellers. The internal flow aerodynamic problems, while numerous, will not be considered here, but the interaction of internal with external airflows will be mentioned briefly as deserving more research attention.

Any air inlet on an aircraft will encounter forces and moments arising from the momentum of the air taken on board, as well as from the classical external aerodynamic forces resulting from the air which remains external. There is of course the ram drag, equal to the mass flow swallowed times the flight speed. This is not a new phenomenon, but because of the much larger mass flow rates required for V/STOL aircraft compared with conventional aircraft experience, inlet momentum forces assume correspondingly increased significance. There will also generally be a moment about the center of gravity, equal to the ram drag times the distance between the center of the free stream tube of air swallowed and the center of gravity. For inlets ahead of the center of gravity, these moments are

destabilizing both in pitch and in yaw. It might also be noted that the moments are directly proportional to flight speed and at low to moderate speeds can be of larger magnitude than the stabilizing moments from the tail, which are proportional to the square of the flight speed. Thus an aircraft with air intakes predominately forward of the center of gravity will experience a regime of static instability at low speeds.

The exit stream tube or tubes are of even greater significance than the inlet flow because of the very large momentum involved, equal to the gross weight in hover. The presence of this downward exit flow in forward flight obviously can greatly affect the external flow along the underside of the aircraft and consequently the pressure distribution on wings, fuselage, and tail, particularly those portions aft of the exit. Much basic aerodynamic investigation of this general phenomenon remains to be accomplished, including studies of the momentum interchange between the exit and free stream flows and the interference forces and moments that are encountered on solid surfaces in the vicinity of both inlets and exits.

Aerodynamic Sound Sources

The noise produced by V/STOL aircraft in hover and low speed can be of critical importance in both commercial and military operations. It is likely that some configurations will be excluded from certain types of operations because of annoyance or detectability resulting from the aircraft noise. While the aerodynamic sources of sound are known in general to be the shearing action between high speed exhaust flows and ambient air, vortex generation, particularly on the rotating blades of the thrust device, and the rotating pressure fields of the same blades, not nearly enough is known about the actual mechanisms involved, nor is there adequate information on techniques to permit designing an aircraft of a particular disk loading to have substantially reduced sound levels. With helicopters, the phenomenon known as "blade slap" is, when it occurs, particularly annoying and detectable. The origin of this sound has not been adequately explained, and an improved theoretical description is needed as well as practical means for avoiding the problem.

Wind Tunnel Wall Corrections

A considerable amount of work remains to be accomplished to develop reliable methods for correcting VTOL model characteristics to account for the presence of wind tunnel walls. Because the downwash momentum is high and the downwash angles become extremely high at low speeds, strong recirculating flows are established that would not occur in completely free air. Practical low tunnel speed limits and/or limits of the

ratio of model size to tunnel size need to be established. Within the useful range of model size and tunnel speed, the work required includes improvements to current theories and correlation with both existing data and additional systematic data acquired for this purpose.

CONCLUSIONS

There is much research work that needs doing in the field of V/STOL aerodynamics before successful aircraft in a categories can be developed.' There are enough of these problems to keep all appropriate segments of government and industry hard at work for many years. Let's keep at it!

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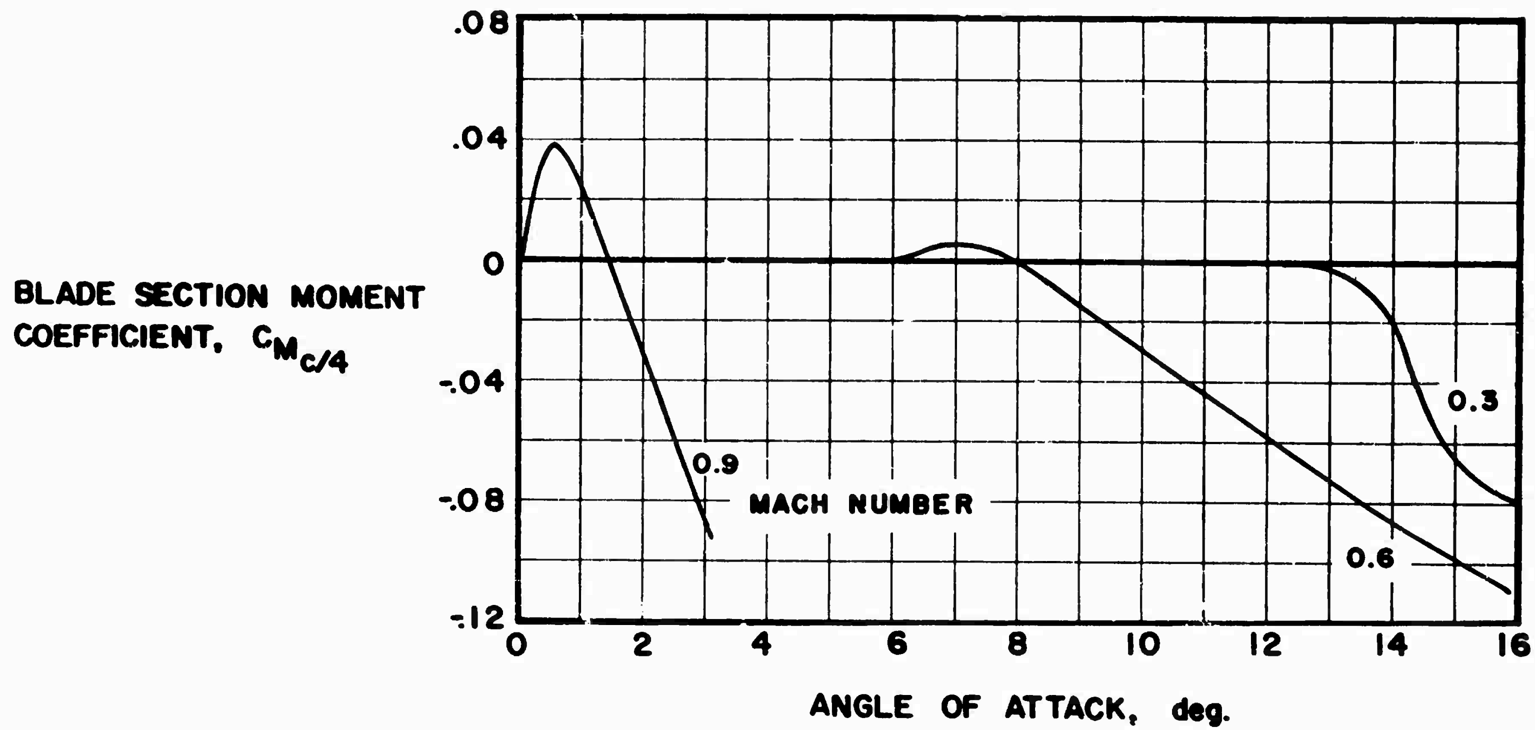

FIGURE I. INFLUENCE OF MACH NUMBER ON AIRFOIL PITCHING MOMENT CHARACTERISTICS.

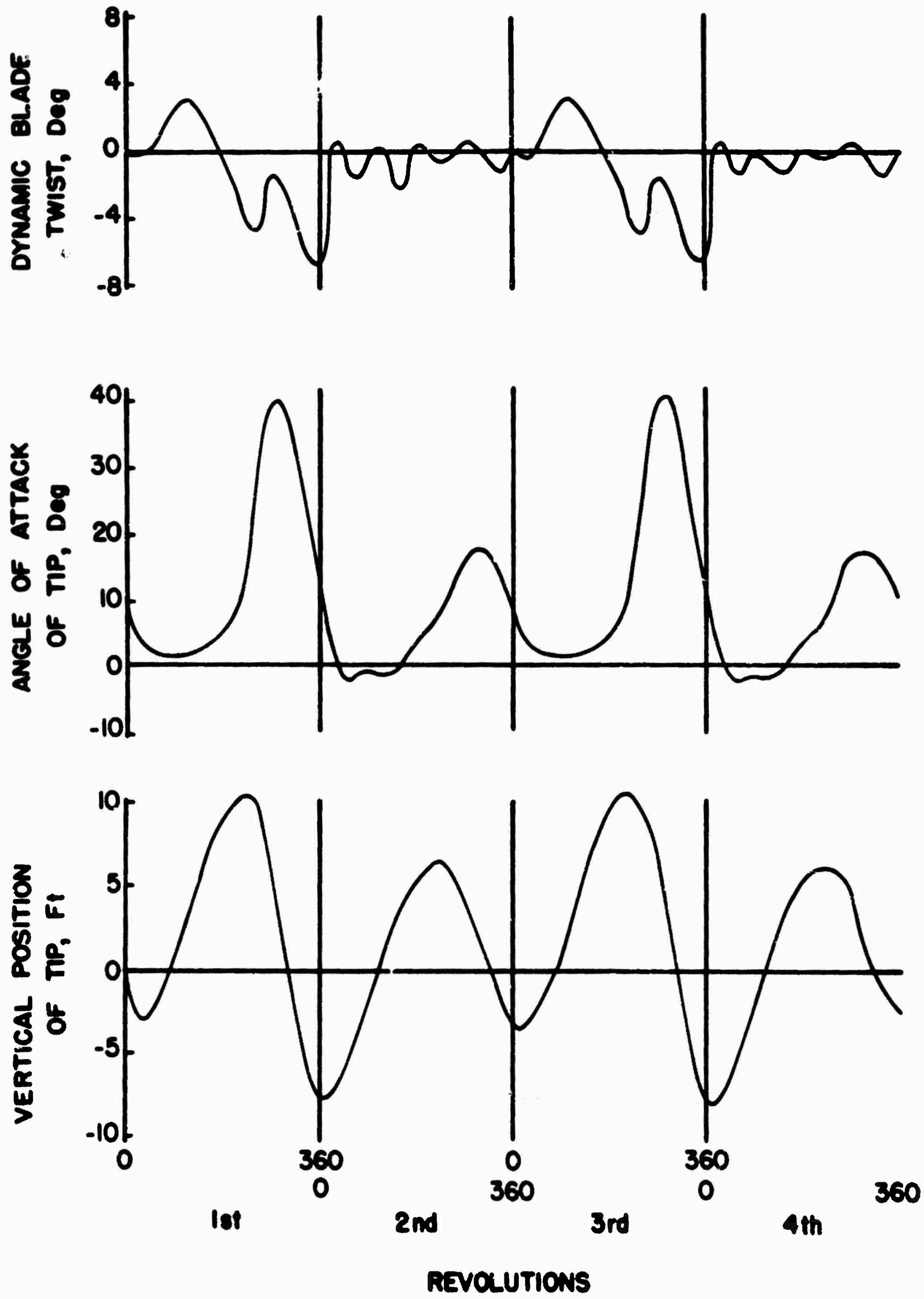

FIGURE 2. CALCULATED BLADE RESPONSE WITH NON-LINEAR BLADE PITCHING MOMENTS AT HIGH MACH NUMBER.

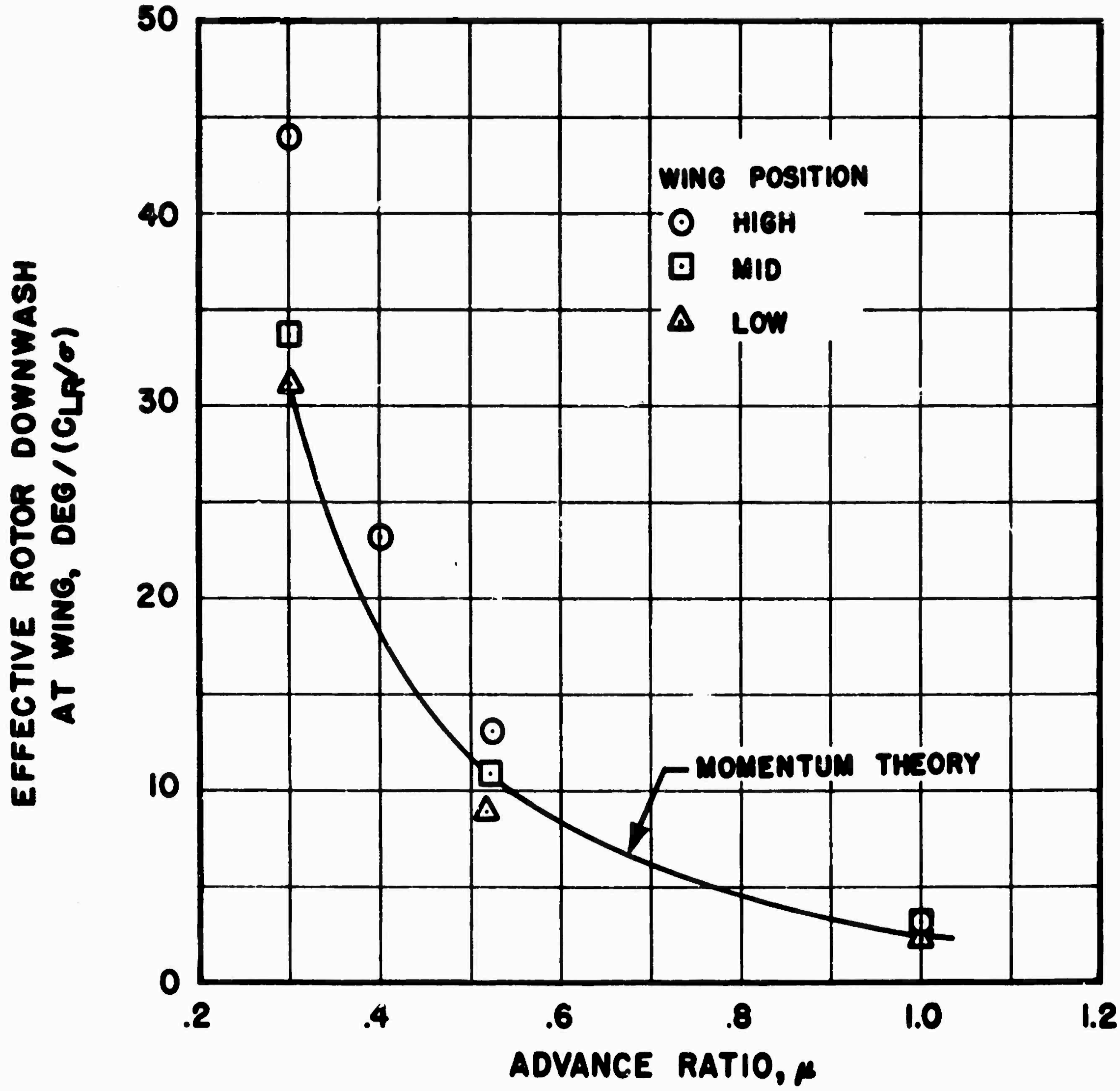

FIGURE 3. EFFECT OF FORWARD SPEED ON EFFECTIVE ROTOR DOWNWASH ANGLE.

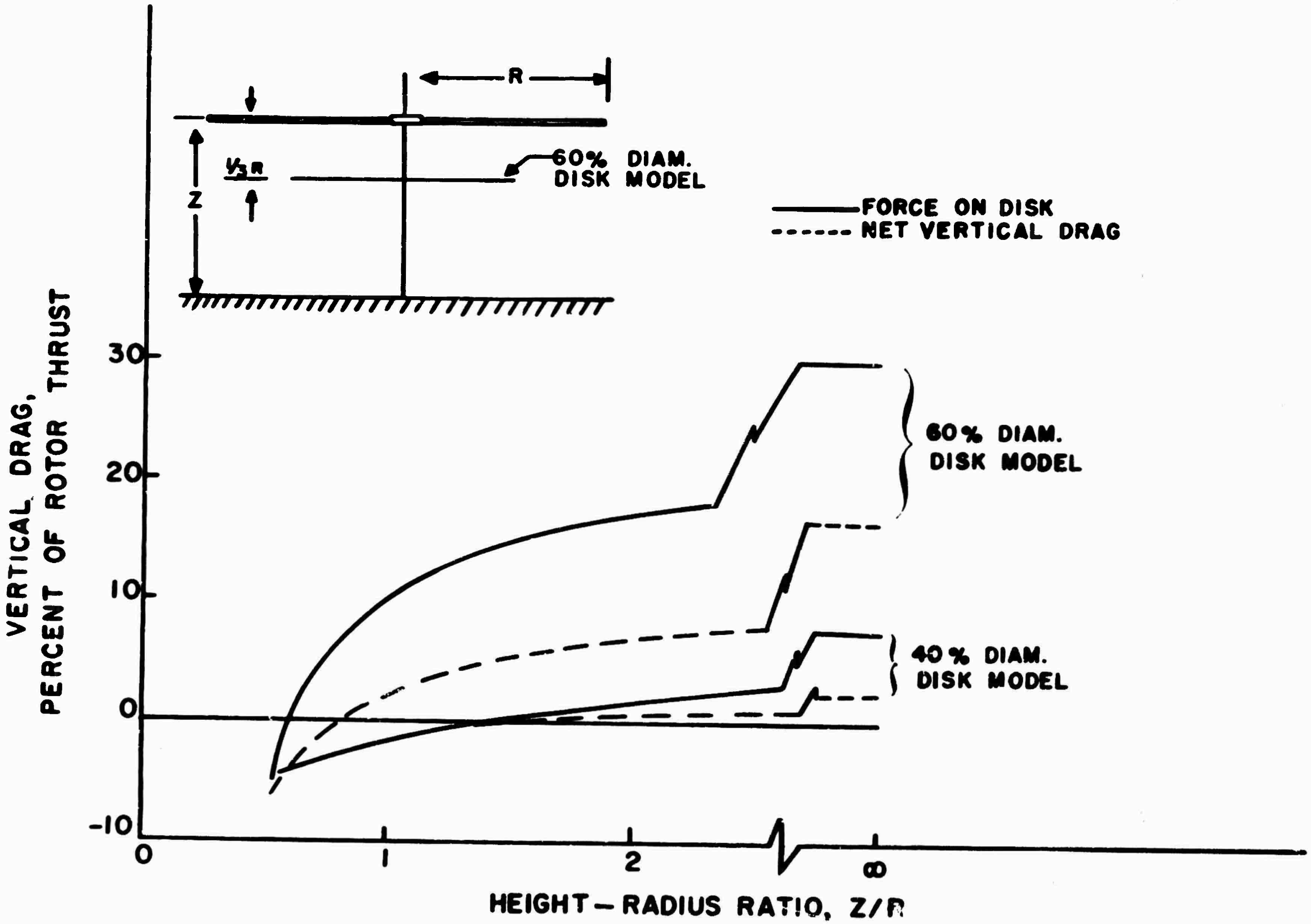

FIGURE 4. EFFECT OF HEIGHT ON VERTICAL DRAG OF TWO DISK MODELS.

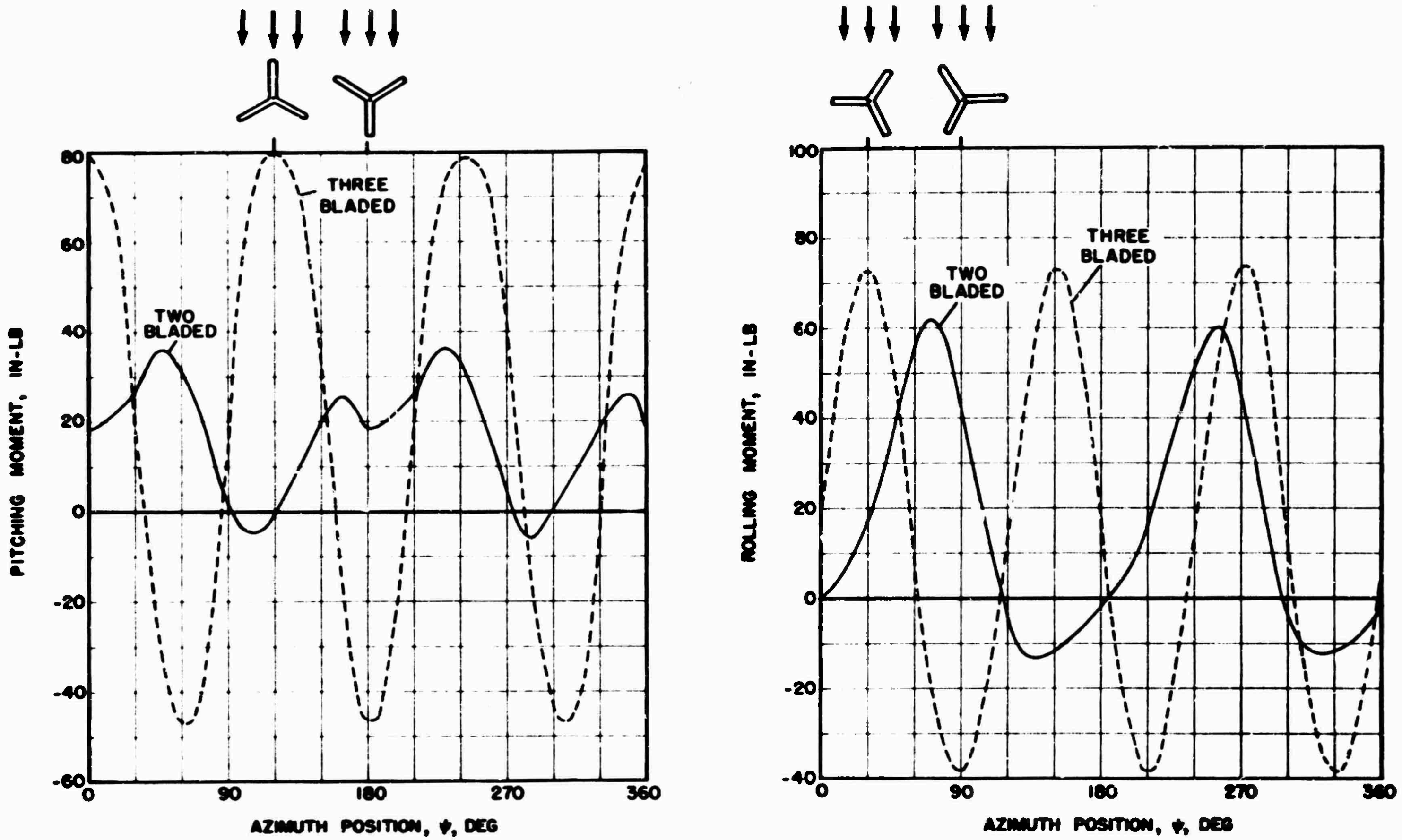

FIGURE 5. STOPPED ROTOR PITCHING AND ROLLING MOMENTS.

LOW SPEED AERODYNAMIC PROBLEMS ASSOCIATED
WITH HELICOPTERS AND V/STOL AIRCRAFT

by

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INTRODUCTION

For several years, the V/STOL industry has been poised on the threshold of significant advances in the performance of V/STOL aircraft. During this time, continued improvement in helicopter productivity has been realized through the attainment of higher operating speeds and better payload/weight ratios. Meanwhile other V/STOL design concepts have been and are being evaluated for their relative merits in fulfillment of V/STOL mission requirements.

Increased speed, range, and payload are fundamental to future requirements in order to achieve increased productivity as well as satisfy specific mission needs. In terms of aerodynamic design, this will require the attainment of higher speeds at the best possible lift/drag ratios together with satisfactory flying qualities throughout the flight regime.

Present efforts are characterized by continuous tradeoff studies of several configurations to optimize and evaluate their relative merits. Decisions concerning configurations are far reaching and the following areas of

aerodynamic research are recommended in order to provide a better basis for the decision-making process.

Helicopter Rotors

Continued development of helicopter rotors appears both possible and worthwhile with the maximum speed potential yet to be realized. Problem areas associated with the development are the attainment of acceptable rotor propulsive efficiencies and the elimination of rotor limitations including blade stall, compressibility, and excessive flapping. The achievement of higher performance may well be secondary to the dynamic problems encountered as a result of the unsteady rotor loads generated at higher tip speed ratios, dynamic pressures, and Mach numbers. Accurate performance and airloads prediction will require that the aerodynamic environment of the rotor be fully understood and accounted for in rotor mathematical models used in the analytical design of high performance rotors.

Continuing efforts at the Vertol Division of The Boeing Company in experimental and analytical studies of rotor systems have led to one such refinement in development of the non-uniform downwash theory reported in Reference 1 and the programming of the method on the computer for rotor analysis and development. Good correlation between theory and test has been obtained in the prediction of rotor performance and in the determination of rotor airloads by means of this method for unstalled rotor flight conditions up to μ's of about .4. The airfoil development program reported in Reference 2 was also conducted and conclusions are reported with regard to improved airfoil sections for rotor applications. Using the airfoils and spanwise airfoil distribution

proposed there together with associated planform, twist, and structural considerations, it is possible - using present theoretical methods - to project helicopter operational speeds to around 200 knots at useful lift to drag ratios.

To be able to precisely configure rotors to operate at higher tip speeds and advance ratios at reasonable L/D ratios, many of the basic assumptions, which have persisted from the past, are no longer acceptable for the higher speed regimes. The mechanics of rotor blade stall and the effects of radial flow and airfoil oscillation on rotor blade section force characteristics still need to be explored. The definition of blade "stall" itself is debatable. Efforts at Vertol in attacking the radial flow question were reported in Reference 3 and provide some insight into the problem, but without final conclusions. At this time, the discrepancy between theory and test seems to narrow down to the contributions of radial flow and/or the difference in force characteristics between stationary and oscillating airfoils. Research in both of these areas is recommended and encouraged. Oscillating airfoil tests appear to offer the nearest term results and should be pursued as soon as possible. The use of oscillating airfoil data, together with today's refined rotor analyses including non-uniform downwash, should indicate if effects of radial flow are also present.

Research of the radial flow question appears to be much more difficult, both theoretically and experimentally, and specific methods of attack are not readily apparent. At least one area that might be explored would be wind tunnel testing of yawed static and oscillating airfoils near stall.

Propulsive Rotors

Advanced propulsive rotor concepts should be examined. One such concept - the segmented rotor - was evaluated by Vertol for USAAVLABS in wind tunnel tests of a model helicopter rotor. As reported in Reference 4, the results demonstrated that the segmented rotor concept was aerodynamically successful in that it was able to develop a high level of propulsive force greater than that possible with a conventional rotor at $\mu = .6$. Structural and mechanical implementation of a similar full scale rotor, however, suggests some need for caution and leads one to consider other, perhaps similar means of achieving the same end result. The use of a jet flap for this purpose was discussed in Reference 5 and represents one interesting approach. In this case, airflow used for the inboard segment flap might also be utilized in boundary layer control of the outboard segment of the retreating blade, as well as drag reduction of the shanks and hub. Recommended research would include the analytical and experimental (model) evaluation of such a concept. However, early investigation of design feasibility would be a necessary prerequisite and comparison of the segmented rotor with other promising propulsive rotor concepts as well as auxiliary propulsion concepts is also required.

Rotor Testing

The extension of the application of present rotor aerodynamic theory to higher speed operating regimes needs further verification with test results. Experimental flight testing of full-scale compound helicopter configurations has already provided considerable worthwhile data and should be continued. Flight test programs of full scale helicopters to measure rotor airloads have also been accomplished and will most certainly prove invaluable to the

aerodynamics engineer as he consumes and begins to understand this wealth of data. These efforts should be continued and the results correlated with theory in order to provide updating of present rotor mathematical models. Toward this end, full scale wind tunnel tests of an instrumented rotor are recommended in order to solidify the results determined from flight tests.

Increased emphasis is recommended in the wind tunnel testing of dynamic model rotors at full scale Mach numbers and tip speed ratios. Inability to wind tunnel test full-scale rotors under such conditions and the cost and elapsed time for full-scale flight testing points strongly to the need to lean more heavily on dynamic model rotor test results. Research into dynamic modeling of rotors is recommended with the objective of using rotor models as design tools in the evaluation of rotor blade airfoil section, thickness taper, planform taper, twist, stiffness, mass distribution, etc. Investigation of compressibility effects and Mach number limitations as well as limitations associated with blade torsional and flapping responses to blade stall might also be amenable to dynamic model experimentation. Model rotor airload data is also desirable for ultimate correlation with full-scale flight and wind tunnel test results.

Other V/STOL Concepts

Promising V/STOL designs other than pure helicopters take many forms. The possible combinations of rotors, wings, propellers, turbofans, lift fans, and thrust deflection devices seem infinite and so likewise are the problem areas and the research needs. A major problem would seem to be in the acquisition of basic data with broad enough application to provide for uniform

tradeoff study results. Such a baseline is required as much by the contracting government agencies as by the industry. With this thought in mind, only a few areas will be considered for the present discussion.

First, the full-scale and model rotor wind tunnel research programs recommended earlier apply equally well to compound helicopters, and those programs should include rotor operating conditions corresponding to the unloaded, non-thrusting rotor of the compound configuration. For the compound, auxiliary propulsion in the form of propellers, turbofans, or any hybrid in between, poses considerable challenge to the aerodynamic engineer in the prediction of relative efficiencies. Fundamental research in this area appears desirable in order to provide the needed data for tradeoff studies.

For propeller-driven tilt wings and tilt rotors, the performance prediction of propellers and/or rotors at high shaft angles of attack presents a real problem. Suitable theory for predicting the aerodynamic forces and moments at and about the hub is still required. The crux of the problem lies in defining the non-uniformly skewed wake, and research in this area is strongly recommended. Also, for the same configuration (tilt wings and tilt rotors), the autorotation of rotors or propellers of low disc loading in the presence of wings represents an unknown area. Again suitable theory and experimentation are required.

Large scale testing to provide further insight into the generation and reduction of download and interference is also recommended. Properly oriented testing in this area can provide for uniformity of download and interference data and thereby enable better evaluation of competitive configurations

in both hover and forward flight conditions. Utilization of auxiliary wing devices in hover, for instance, can substantially reduce download on wings located in the propeller or rotor slipstream and provide improved hovering efficiency of V/STOL types. These same devices can also serve to improve $C_{L_{max}}$ during other modes of flight.

Flying Qualities

It would hardly seem proper in a discussion of V/STOL aerodynamic problems to close without some direct indication of concern for continued V/STOL flying qualities research. It is probably worth noting in this regard that the rotor and rotor/propeller research programs already recommended also need to provide derivative data and whenever logical should include the determination of characteristics under dynamic as well as static conditions. Finally, the evaluation of V/STOL flying qualities such as that planned in the X-22A variable stability program should be extremely valuable in determining the requirements for the design of the next generation of V/STOL aircraft.

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SELECTED RESEARCH RESULTS

and

RECOMMENDATIONS FOR AERODYNAMIC RESEARCH

by

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INTRODUCTION

Some results of research for a particular V/STOL approach are reviewed. Several areas of more general aerodynamic research are recommended.

Lockheed's active entry into the rotary wing-V/STOL field was based on the philosophy of combining the fundamental hovering efficiency of the helicopter rotor with a simple mechanical means of achieving good flying qualities. This concept and subsequent developments for higher speed flight were initiated with extensive company independent research. The United States Army and Navy, and NASA have supported and extended wind tunnel testing and flight exploration of several promising developments.

THE LOCKHEED RIGID ROTOR

The Lockheed rigid rotor system incorporates cantilever mounted blades of tailored root stiffness. Feathering control is achieved by the precession of a mechanical gyro which is integral with the rotating swash plate. The precession accompanies unbalanced moments about the gyro roll and pitch axes which are produced by pilot inputs and blade root moment

feedback. Pilot stick motion is transmitted as a spring cartridge pressure to the periphery of the non-rotating swash plate. Blade root moment feedback is transmitted through the feathering linkage between the gyro and the blades on the basis of a design selected linkage ratio. These features combine to provide high control power and self-correcting response to upsetting disturbances.

THE XH-51 RESEARCH PROGRAMS

A year and a half of flight testing of this principle on the Lockheed CL-475 helicopter led to the further application at higher speeds on the Navy and Army funded XH-51A helicopter. Extended maneuver envelope flight testing on 3 and 4 blade versions of the XH-51A helicopter and higher speed flight testing with the compound version of the XH-51A have been accomplished under Army programs. The helicopter and the compound versions have demonstrated rotor operation at stabilized speeds of 175 kt and 237 kt, respectively. This flight research has now been broadened. Preparations are under way to obtain blade chordwise and radial pressure measurements for the established flight envelopes and to extend the XH-51A compound helicopter flight envelope by decreasing the rotor rpm.

Lockheed AAFSS Research

Lockheed began direct anticipatory research for the AAFSS vehicle late in 1962. As a point of interest it may be noted that 60 distinct configurations, some with several variations, were ultimately considered and evaluated in the Lockheed tradeoff studies.

Hub Drag Reduction

Clean fuselage lines and a retractable skid gear had been incorporated in the XH-51A helicopter to permit good speed potential. To take greater advantage of the absence of flapping and drag hinges on the rigid rotor, AAFSS effort was directed toward minimizing the rotor hub and control gyro drag.

A one-half scale, wind tunnel model was constructed and tested. A starting refinement included low profile blade root arms and feathering bearings. From this initial point, further cleanup in hub and gyro design was considered. Potential modifications of the gyro ranged from a minimally modified external configuration to a completely submerged design. The tests produced a low-drag hub and external gyro which is applied in AAFSS. This configuration was selected as an optimum for the many requirements including maintainability and performance. Principal drag reduction gains were made in sequence by minimizing the rotating frontal area of individual components, rounding edges and smoothing contours, and reducing gaps. The full scale drag improvement corresponds to a parasite area drag reduction of 3 square feet. Also, an additional reduction in power to turn the rotor was realized. This reduction ranged from 8 hp in hover to 36 hp at high speed.

Propulsion and Yaw Control Systems

The AAFSS research effort included optimization of a propulsion and a yaw control or anti-torque system. Among the configurations considered prior to the final selection of a separate pusher propeller and a tail rotor were the swing prop-tail rotor combination and the vectored-thrust pusher propeller.

Two features of the swing prop-tail rotor appeared attractive. In hovering, no separate idling propeller would be absorbing power, and at high speed, no separate tail rotor drag would be incurred. Two of the counter features favoring the separate pusher propeller and tail rotor units are pertinent. Detail design showed the fixed shafting and separate airscrews to be lighter. The required vehicle operational flexibility for horizontal acceleration and deceleration over the speed range showed the separate pusher propeller and tail rotor to be more favorable. Each could be called upon to perform as necessary without a compromise in its performance at any stage in flight. No maneuver degradation would be incurred due to the need for changing the position of the airscrew axis.

The vectored thrust pusher propeller seemed to offer some advantages in weight and in mechanical simplicity in the early stages of consideration. The simplest form of this assembly consisted of a vertical surface with leading edge slots and a 35% chord flap. The surface was located behind the propeller to provide side force with flap deflection. The configuration evolved to a biplane vertical surface and ultimately to a ducted assembly. The most effective configuration showed the following results. At the side force level for anti-torque control in hovering, the vectored thrust arrangement requires 30 percent more power than the tail rotor. To hover in a 30 knot tail wind, the vectored thrust arrangement requires five times the power of the tail rotor. These factors are among those which led to the separate tail rotor and propeller configuration.

SLOWED AND STOPPED ROTOR VEHICLE RESEARCH

Lockheed research into higher speed rotary wing aircraft encompasses slowed and stopped rotor applications. These continue to reflect a blending of aerodynamic and structural dynamic concepts. Principal research areas from inception to feasibility have been Lockheed funded. An interest in further development had led to significant support by the Navy, which has been extended by the Army and NASA.

Minimizing blade loads from the high rpm to the stopped condition is accomplished by a special form of swash plate control monitoring. At high rotor rpm, control gyro monitoring is high in authority. At low to zero rpm, an aerodynamic vane is employed to actuate the swash plate position and thereby minimize blade loads. Relative authority of the gyro and the vane is scheduled by rpm sensing.

One interesting result obtained in stopped rotor research is that regarding the effect of blade arrangement on drag. Tests show that a stopped rotor with the blades folded aft does not necessarily produce less drag than a stopped rotor with the blades in the untrailed position. The desirability of relieving blade airloads does nevertheless make folding desirable. The full benefits of the folded rotor approach are achieved

with stowing the rotor in an integral airframe fairing.

MORE GENERAL AREAS OF ROTARY-WING V/STOL RESEARCH

The application of various design concepts and research results to operational vehicles makes it evident that several general areas require continued research. These encompass basic and applied aerodynamics, and of these, several are closely related. They are:

1. Airfoil environment in the rotor flow field.
2. Rotor maneuver load factor in non-propulsive flight.
3. Rotor airfoil geometry limits for design tradeoff.
4. Airfoil contour tolerances and blade erosion.
5. Aerodynamic characteristics of dual airmobility-ground weapons.

Airfoil Environment in the Rotor Flow Field

A more complete understanding of airfoil behavior in rotor flow field environment is needed for operation near airfoil stall. This information is required for tailoring rotor blade design to stringent performance and handling qualities requirements. It is also needed for predicting structural dynamic characteristics.

Investigations of the last few years have more fully described induced velocity variation. Chemical film flow visualization has been used to describe boundary layer flow at the blade airfoil surface, and has been followed by some pressure probe work to explore the boundary layer velocity profile. Chordwise rotor blade pressure measurements at selected radial stations have been obtained. These indicate a change in local airfoil angle of attack due to vortices shed by a preceding blade. In general, the new analytic and experimental results for unstalled conditions indicate good agreement with two-dimensional data if the angle of attack in the rotor flow field is adequately defined.

The influence of lift and moment hysteresis accompanying airfoil

stall has been examined in conjunction with blade dynamic behavior, and reasonable correlation has been obtained. An area of disparity occurs however, in regard to two-dimensional airfoil performance. Rotor wind tunnel tests indicate a continuation of airfoil lift beyond the two-dimensional stall angle. It appears that the drag in this regime increases as rapidly or more rapidly than that in two-dimensional flow. Further research is necessary to explain these contraditions. Establishing the character and contribution of the boundary layer, describing the time dependent characteristics of the stall flow field, and correlating the results with pressure measurements remain to be accomplished.

Rotor Maneuver Load Factor in Non-Propulsive Flight

Although varying induced velocity is helpful in defining rotor loads prior to airfoil stall, performance limits in the proximity of stall are not necessarily more clearly explained by consideration of varying induced velocity. For helicopter propulsive flight, the varying induced velocity treatment indicates that retreating blade stall tends to occur more inboard; somewhere between the tip and the reverse flow region rather than at radius stations outboard of 95 percent. This more inboard high angle of attack region corresponds closely to that described with uniform induced velocity for a rotor carrying a high lift load but not producing a propulsive force. The latter flight condition may be demonstrated by a compound helicopter rotor pulling normal load factor beyond wing stall. Model rotor wind tunnel tests indicate that a rotor may be flown farther into regions of airfoil section stall if no rotor propulsive force is required.

As is the case for the conventional helicopter rotor, that of the compound helicopter may be subject to high speed advancing tip Mach number limitations. Retreating blade stall, however, would more likely accompany maneuver normal load factor at moderate advance ratios rather than one g flight at high advance ratios. Tapered thickness rotor blades are suited to alleviating advancing tip Mach number, lighter total rotor weight, and greater latitude in adjusting rotor frequencies. It is, therefore, de-

sirable to establish the extent to which thinner sections may be applied in non-propulsive rotor operation before lift degradation occurs. For lack of adequate theoretical guides, it is recommended that aerodynamic flight research be conducted to more clearly define the lift limits of rotor operation with thin airfoil sections in non-propulsive flight.

Rotor Airfoil Geometry Limits for Design Tradeoff

Over the years, NACA-NASA direction has indicated that far-forward airfoil camber is useful to increasing airfoil maximum lift coefficient without causing severe pitching moments. Whirl testing by NASA has included 63A-series airfoils wrapped around 130 and 230 mean lines. Further two-dimensional tests of the uncambered 63A-series airfoils have been performed under Army direction to provide baseline data for this airfoil family.

More recently reported work of industry conducted research includes results of nose thickness on maximum lift coefficient and on compressibility drag rise. A selected airfoil geometry is recommended for application to a helicopter rotor. These airfoils embody thickness ratios between 6 and 10 percent, thick nose sections, and far-forward camber. The possibility of over-cambering the nose sections in conjunction with a selected blade twist is noted.

Although it appears that the limits of rotor performance improvement through airfoil selection are near at hand, it is necessary to more clearly define the limits of interchangeability of thickness ratio, nose thickness and airfoil camber as they may be applied to a blade selected for the conventional or the compound helicopter. These guides are pertinent to careful matching of the blade to the flight envelope and to the dynamic characteristics. Further airfoil and rotor testing are recommended to define these guides.

Airfoil Contour Tolerances and Blade Erosion

The design of a high performance conventional or compound helicopter

with a given installed power requires careful monitoring of the available power to meet the performance requirements. Power losses due to compressibility must be minimized, and accordingly, it would appear that close contour tolerances should be employed. An examination of the tolerances to which rotor airfoils are currently fabricated is of interest. These tolerances in relation to airfoil thickness are generally coarser than those which are considered satisfactory for the wing airfoil sections of high speed fixed wing aircraft. Carefully controlled whirl test and large scale wind tunnel tests are required to evaluate the effects of contour tolerances on hover and forward flight rotor performance.

A second phase of such effort is in part reflected by the testing of leading edge erosion strips for the evaluation in resisting contour deformation. Such effort has been conducted under the direction of Army Aviation Laboratories over the past few years. A major aspect of this type of testing should include a careful evaluation of rotor performance degradation due to contour deterioration accompanying erosion. Here again, carefully controlled tests are required to evaluate rotor performance degradation. Useful information could be obtained with both contours that have been actually subjected to erosion and those incorporating simulated but controlled deterioration.

Aerodynamic Characteristics of Dual Airmobility-Ground Weapons

Benefits of rotary wing V/STOL aircraft in Army airmobility are being demonstrated at this time. Simplicity in logistics has dictated the application of several ground weapons for use in aircraft being employed. It is perhaps an indication of the strides that have been made in the performance of such aircraft that they can accommodate and employ existing items of special equipment. These items are heavier, of greater drag area, and subject to greater airloads than they would be had they been initially designed for both applications. Considerable strides have been made with regard to equipment which is now intended to be applied for both ground and air use. In considering the cost of the air vehicles which are in use,

and the techniques of making them more effective, the need for improving the aerodynamic characteristics of the dual airmobility-ground weapons is evident. It is recommended that greater emphasis be made in achieving drag reduction and minimal airloads for dual airmobility-ground weapons.

CONCLUSION

Some developments of a particular approach of rotary wing V/STOL aircraft have been reviewed. Several areas of more general aerodynamic research have been recommended. It is not surprising that a large portion of these are associated with rotor design. These include:

1. Obtaining a better understanding of airfoil environment near rotor operating limits.
2. Acquiring additional test data to establish operating limits where theory is inadequate.
3. Obtaining practical definitions of performance losses associated with airfoil contours as influenced by fabrication tolerances and erosion deterioration.

Finally, the importance of adequate aerodynamic research is emphasized for improving the air application of dual airmobility-ground weapons.

RECOMMENDATIONS FOR AERODYNAMIC RESEARCH ON HELICOPTERS AND V/STOL AIRCRAFT *

by

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1. HELICOPTERS

It is perhaps presumptuous for me to lay down research requirements in the field of helicopter aerodynamics since considerably more work has been done on rotorcraft aerodynamics in the U.S.A. than in the U.K. However some general remarks may not be amiss.

The two main problems are generally reckoned to be

(a) decrease of vibration and noise

(b) to provide an increase in forward speed (in order to extend the future operating roles of helicopters).

In the long-term, progress with (a) involves a much deeper understanding of all aspects of rotor aerodynamics and aero-elastic effects so that we may be able to minimise the aerodynamic forcing of vibrations. But there is one school of thought which maintains that more rapid progress is likely to be made by devising mechanical means of suppressing oscillations at the rotor, and by isolating the cabin.

The possibilities for an increase in speed involve research into

(a) drag reduction
(b) lift compounding
(c) thrust compounding
(d) increasing rotor efficiency

(e) elimination of tail rotor

(f) improving stability and controllability

It is difficult to place these in order of priority since so much depends on the type of design required i.e. on the particular helicopter role desired.

Some particular research items which can be recommended as likely to be most fruitful in improving our general understanding of rotor aerodynamics are listed below, but the list is by no means a complete one.

(a) continued computor studies on rotor aerodynamics, particularly the interaction with structural effects, in order to obtain better correlation with test results. A programme being developed in the U.K. uses an iterative process (with only linear aerodynamics so far) and will be verified by comparison with test data.

(b) if such a programme were to prove satisfactory over the regions where flight data are available, then exploration of regimes beyond these are possible. These include higher speeds, higher C_L's, transition of tilt-rotors etc., and it is hoped such investigations would lead to a better appreciation of the sources of vibration, determining limitations, study of transition effects, stopping of rotors in flight etc.

(c) Continued studies of the interference of lifting surfaces with rotors, e.g. periodic stresses caused by passage of a rotor close to a stub wing, blockage effects etc.

(d) more intensive studies towards improving the choice and distribution of aerofoil sections for rotors in order to reduce drag, to delay compressibility effects etc.

(e) In general, continued attention to improving $^L/D$.

2. V/STOL AIRCRAFT

Perhaps the most important requirement in this field is to produce fuller comparisons of wind-tunnel and flight data, particularly with

representation of jet efflux in the tunnel. This would have repercussions over the whole spectrum of V/STOL aerodynamic research.

Performance estimation is also a vital problem since experience has shown that it is often the last 1% or so that is vital in practice.

Flight experience in the U.K. has also shown the need for more work on the laws for control power requirement, in particular

(a) the angular acceleration required for manoeuvring (this can be extracted from flight work)

(b) the control power required for trim change - this can be estimated from wind-tunnel data, so the need for more tunnel work is emphasized.

(c) the control power required to counteract disturbances (such as gusts)

In general, more work is needed on the handling characteristics of any type of V/STOL aircraft, and on the flight boundaries e.g. stall boundaries in steep descent, indeed the operating boundaries over a wide range of lift.

In the direct jet-lift field of V/STOL further effort is needed on the study, at zero and low forward speed, of ground erosion effects and aircraft performance changes near the ground. This need has been strengthened by the current interest in jet nozzles designed to minimise ground erosion effects

The aerodynamic interference of the lifting jet flow on the airframe in the transitional phase from jet borne to conventional flight have been studied in considerable detail in the U.K. during the past six years. (Refs. 1 and 2). Apart from defining the magnitude of the interference loads for a variety of possible aircraft configurations, this work has yielded a much clearer understanding of the physical mechanisms and the relative importance of the various parameters involved. There is scope

for considerably more experimental and theoretical work in this field.

In the field of STOL, there is a need for an improved understanding of the aerodynamics of a high-lift wing, both from the sectional design aspect, and for the complete aircraft. The wing sections which we are considering have either plain flaps with artificial boundary-layer control, or a slat and multiple slotted flaps. We hope to be able to achieve this better understanding by first establishing reliable potential flow solutions for such high-lift sections. By comparison with experimental data, it may then be possible to establish a prediction method which allows for the effects of viscosity.

For the complete aircraft we need to refine our prediction methods (particularly on the drag aspect), for the effects of spanwise discontinuities of the high-lift devices and for the presence of the fuselage on the performance of the high-lift wing. Again this must be done by correlating results from an inviscid theory with carefully controlled experimental measurements.

Ref. 1. R.A.E. Tech. Report No. 65065. Recent Basic Research on V/STOL Aerodynamics at R.A.E. By J. Williams.

Ref. 2. Aerodynamic Interference Effects with Jet-Lift Schemes on V/STOL Aircraft at Forward Speed. By J. Williams, M.N. Wood. Paper for AGARD Specialists Meeting on Aerodynamics of Power Plant Installation. Oct 1965

PANEL II

QUESTION AND ANSWER PERIOD

CHAIRMAN CHEILEK: Because our discussion is limited to 15 minutes, unless someone on the Panel has an immediate comment, I would prefer to ask for discussion or recommendations from the floor first.

QUESTION: Frank [D.] Harris, Vertol Division of Boeing.

I am somewhat disappointed in the Panel . . . let me clarify [that]. I have had the impression, getting closer to some of the rotor problems, that it is time for the low speed aerodynamicist to contribute to the control load problems that helicopters meet. My notes don't indicate that anybody talked about it. Perhaps each one of the Panel members could comment on our relationship to control loads, or at least their opinions of that situation.

MR. DREES: The matter of control loads [was] pointed out in my discussion as [being] of some concern at high speeds. The tests that we did with a high performance helicopter at speeds over 200 knots indicated the two areas in the rotor — at least for the rotor we investigated — that cause high control loads.

The first area is on the advancing side, just past the azimuth angle of 90 degrees, where we see a very sharp spike coming in on the pitch-link loads when we fly at high advancing tip mach numbers. The second area could be identified with control loads induced by the reversed flow area, where, of course, the blade is approached from the wrong side and the aerodynamic center is way back on the blade.

By using the thin tips, we could — in this same program for AVLABS — see a definite reduction of the control forces due to compressibility. Other flights connected with this inboard phenomenon of reversed flow area indicated that we do see loads also by cutout.

In general, the control load problem is a very sticky one for predictions. Our feeling is that, except for flight conditions where we have very high advancing tip mach numbers or a large reversed flow area, the oscillatory blade bending and oscillatory loads on a deflected blade are causing larger control loads than the aerodynamic control moments. I think that concludes my contribution.

CHAIRMAN CHEILEK: Would anybody else on the Panel like to address themselves to this subject?

MR. FRIES: I think at the risk of practicing a little gamemanship, I would like to have Frank Harris comment on the work we have been doing at Vertol. Specifically, Frank has been doing the work, and I'm not about to steal his thunder. So, why don't you comment in relation to the things Jan Drees has already said, Frank?

MR. HARRIS: I didn't expect to be put on the spot like that. Yes, we have been working on the problem, and I certainly agree with Jan. Particularly in some model tests that we have run, you can get one heck of a load due to pitching moment when this blade runs up at very high tip speeds and this pulse of load comes in the advancing side. In addition, as Mr. Drees pointed out, we can identify a load that comes from the reverse flow region.

The thing that we found is that there is, very obviously, a flutter load that precedes at roughly the blade first torsion. [We] probably could live with all of the other control loads, but this one comes up very quickly. I think our model tests have indicated that finding rotor limitations due to flutter or due to control loads in model tests are conservative. Perhaps the most stimulating thought is [that] they appear to go away the faster you go.

I am very curious to know what everybody else thinks about this problem. Because, to me, that is the blade stall problem. If we could lick the control load problem, I think we might just get rid of blade stall.

MR. GORENBERG: Basically, I think each of us has probably been exposed to some of the effects of the control loads and, perhaps, somewhat differently depending on the particular rotor system. I would ask if Frank could mention something about the effects [that he] got with [his] droop nosed blade on the advanced geometry . . . airfoil for example. To what extent did you find the mach number effects alleviated? How much of a gain in advancing tip mach number?

I would like to mention that I ask this because Abe Adler asked me about it, and I couldn't give him a real explicit answer — I had some feel for it, but not quite that recently.

MR. HARRIS: This is sort of getting like one against seven, here; I don't know how long I can last. I [will] just reiterate my position that control loads that arise due to compressibility are probably not the most important ones, and that, with just a little bit of tailoring on the trailing edge portion of the airfoil, you ought to be able to work your Lockheed XH-51 up to 230 knots with no big problems — or however fast your plane will go.

MR. GORENBERG: As far as the reverse flow region and the stall region, every indication that we have from any published model data is that you can go up to regions that would normally appear to be high C_L regions. This whole area seems to be less critical on model rotors and, [with] what limited information I could find, on full scale rotors. In fact, if the span-wise flow effort that you point out has any bearing, I could hardly see any limitations for the propulsively unloaded rotor, because the conditions are sufficiently different to alleviate the principal problem that you run into with the propulsive rotor. The question arises though as to what extent you have to retain C_L and, of course, these are areas we are trying to determine ourselves. That is the only thing I could add to your original statement.

CHAIRMAN CHEILEK: I think Fradenburgh wants to get in on this discussion.

MR. FRADENBURGH: Just briefly. The control load problem certainly is complex, because it involves the aerodynamics of the airfoil and the aeroelastic characteristics of the bending of the blade.

However, there is some work that is being done — we are doing some at Sikorsky, part of which is sponsored by the Army. We are investigating a number of aeroelastic stability phenomena of rotors, and we hope that a lot of this work, which will be published one of these days, will help the industry in their control load problem.

Incidentally, Sikorsky has been fairly successful in the last couple of years in predicting control loads. We haven't been successful so far in designing rotor systems that don't have any.

CHAIRMAN CHEILEK: Did you have a word on this, Abe?

MR. ADLER: I want to point out that we are old fashioned — we are the last ones to have a production machine with a manual control system. It's an 0015, which makes the problem worse.

So, I would like to ask Frank a question, again. If we go back to two-dimensional data, we feel there seems to be reasonable correlation [in] looking for the drag divergence mach number as an indication of when we start looking for pitching moment troubles. I just wonder, with all of the work that you have done recently, [if] you have come across anything that changes that?

MR. HARRIS: No. As a matter of fact, although I didn't show a slide in that radial flow paper this morning that indicated the pitching moment characteristics of this pseudo yaw at infinite wing, the pitching moment characteristics were identical to the drag characteristics, indicating that, regardless of the skew angle, the pitching moment wanted to break away at an angle of attack of 12 degrees.

I am familiar with your CQO type of criteria to a rough extent. It would seem to me that you should — and you indicate success — expect that

that would be a very good criteria, because it correlates with a pitching moment — and that probably is the kicker in this thing — and $C_{L_{max}}$ be damned, so to speak.

CHAIRMAN CHEILEK: We have five minutes [left for this session]. Are there any other comments?

QUESTION: [A. Z.] Lemnios, Kaman Aircraft Corporation.

I would like to change the subject a little bit. I would like to make a practical recommendation for all of these air loads prediction techniques. I am in agreement with all of the Panel members in that a lot has been done, but I also have to deal with the poor helicopter designers. They, it seems, always want information yesterday. All of these prediction techniques are quite complicated, and, by the time you get any useful information to them, it is too late.

I would like to recommend or I would like to see somebody do some work on experimental design techniques. That is, with the large number of parameters that are now used in all of these prediction techniques, what combinations should you use to come up with an optimum blade.

If you cross plot or run a parametric study, by the time you get through with all of these, you may come up with a volume of data that is two-feet high, and no one knows how to use it anyway. So, I would like to see someone use some of these optimizing experimental design techniques. Comments, if you have any?

CHAIRMAN CHEILEK: I am sure everybody on the Panel would agree with that objective. Does anybody want to comment? We have another question back here.

QUESTION: [F. N.] Piasecki, Piasecki Aircraft Corporation.

[I have] just a detail question [for] Mr. Gorenberg; he showed a slide — [it was] the last one he showed — [of] a biplane tail for antitorque purposes, and it apparently was facing upstream in the tunnel, from the streamlining structure shown in the photograph. If that is so, could you give us some information about it?

MR. FRADENBURGH: In that particular position, it was being tested for a tail wind, and it was the result of that test that I mentioned on some of the powers that were required for the particular flight condition. Is there something further?

MR. PIASECKI: No.

CHAIRMAN CHEILEK: One more question.

QUESTION: Jack [Anton J.] Landgrebe, United Aircraft Research Corporation.

There has been a lot mentioned [during] the last couple days, and by the Panel members briefly, on variable in-flow. I think we all agree that it is important. My question is, where do we go from here?

Abe [Adler] mentioned the fact that, well, maybe we should take the existing methods that we have and start using them. Our experience at the [United Aircraft] Research Laboratories in comparing many of the differen theories — including those of Cornell [Aeronautical Laboratory, Inc.], Professor [Rene H.] Miller [of] M.I.T., Mr. [M.A.P.] Willmer of [Royal Aircraft Establishment] England, a few others, and our own — has shown that we can get good qualitative agreement in steady flight, but, at this time, that appears to be about it.

I think it was pointed out very well yesterday, in some of those movies and pictures we saw, that the location of this vortex or of the actual wake geometry appears to be the crux of the matter. What we have really developed in the last few years is a tool (in these computer programs) to take a given wake geometry and find the variable in-flow at the rotor or propellers due to a certain given geometry.

So, I think the wake geometry is the big problem right now. I think quite a bit of effort should be done in this regard to actually give us some experimental idea of what this wake geometry really is.

The papers yesterday, plus some other studies that have been done by Robin [B.] Gray and a few others, have given us a limited amount of information in this regard. But, I think it is time to get into a more extensive effort.

I think possibly NASA or the Army or someone could sponsor an effort which initially . . . well, it could be broken up into two parts — this is my own suggestion; I would like to see what the Panel suggests — [where] initially, [one could] look at the techniques that have come up within the last few years as far as smoke visualization and all different types of flow visualization, analyze these techniques, and try [to] decide which is the best one and, also, what do they really show.

After this study is completed, then make a full-fledged effort to try and come up with a wake geometry picture for various rotors and propellers, different sizes, and run through a parametric study. Maybe we could see [from] all of this a general trend, and that would give us something to work from as far as the input to these computer programs. From that time, we could then go on and get — hopefully — better performance and stresses.

CHAIRMAN CHEILEK: I would assume that several Panel members, when they were alluding to the need for basic aerodynamic research, had in mind, in effect, the kind of program that you discussed in great detail. But, does anybody on the Panel want to comment further?

MR. DREES: Briefly! I think I agree; but I think that we are not there yet. In the first place, there are too many unknowns right now [to permit us] to say let's streamline this. I think the two approaches that we should take on this [are] that we pursue the investigation in the theoretical approach in more detail than we do now; and the other approach I would recommend is that we look for an engineering solution to this complex problem that we can use in our daily life without losing too much computer time [and a solution] that will give us reliable correlation with test data.

CHAIRMAN CHEILEK: Frank Davenport, did you want to comment on this also?

QUESTION: [F. J. Davenport, Boeing-Vertol].

Thank you very much; I did. I feel that while the Panel and many of the speakers have brought up some real important deficiencies, I think we already do have an engineering tool.

To illustrate this . . . in the past, when we used uniform in-flow theoretical methods to predict aircraft performance, we had to "fudge" the drag data to get the answers to square out; and then [we] never would "fudge" it over the whole speed range. So, we were always having to change our drag levels and what-not to explain peculiar performance effects.

But, we found that, using variable in-flow computations — as an engineering tool — we can now routinely predict our aircraft performance, at least up to speeds like 160 [or] 170 knots which is a little beyond where we are actually flying. Our wind tunnel model data and our flight test data on our current aircraft are now predictable, without "fudging" the drag data, using variable in-flow.

Now, once you've defined a configuration, you can do the rotor performance calculation sort of once-and-for-all and have them on families of curves, so that your problem in computer time is vastly reduced. So, I do feel we have made a real stride in making it an engineering tool.

The objections — especially, Where is the wake? — are quite important when we are not talking about real high speeds. When we are getting down to low speeds or if we are dealing with problems in vibration, acoustics, or stresses, well, the objections have a lot of weight.

But, I feel [that] in performance, at least for those companies who are fortunate enough to have large-scale computation facilities and aren't too afraid of using them rather heavily, the thing has been reduced to engineering practice.

CHAIRMAN CHEILEK: Thank you.

QUESTION: [Henry R.] Velkoff, Ohio State University.

I would just like to support the gentleman from United Aircraft [Research Laboratory]. I have been privileged to work with the Army [during] the last few months to review areas of potential research, and [I have] talked to many of you people here, as you surely recall.

And, as Frank Davenport just mentioned, in some areas, the nonuniform in-flows have resulted in a marvelous stride. There are some others, in the stress analysis for example, where the higher modes are not predicted, and this is an area of considerable concern, because, of course, this usually deals with vibration phenomena which is of customer acceptance and as well as the stress phenomena likewise involved.

I think one of the most interesting facets of the things that I have seen here in the last two or three days dealt with these few pictures of [Jeffrey P.] Jones, where he showed the vortex come down and impinge upon the pylon. [Referenced paper contained in Volume I of the Proceedings.]

The strong feeling you get from talking to anybody in this area is that the local flow field — the details of the flow field underneath the rotor in various flight conditions — is going to be all-pervading throughout almost everything we see in our research in the next few years. So, I think it is pretty obvious that we have to continue in this area.

As a matter of fact, we have [to solve] an even dirtier problem: Where are these detail things going? — When they impinge upon the surfaces below them, what do they do to the boundary layer at that spot? — What is the propagation of that thing? — and so forth.

It is a big area, and I am sure that, in the years to come, we are going to see more in this area.

CHAIRMAN CHEILEK: [We have time for] one more comment.

MR. FRADENBURGH: I would like to recommend — I guess I should have had this in the prepared part of my talk — I think we ought to do a little more in the way of flow visualization techniques. We see smoke trails and condensation trails and balsa dust, but maybe we ought to try confetti, helium-filled balloons, and soap bubbles and anything else [that might appear advantageous]. I think this is a very important area. We have to know what that air is doing before we will ever know what is going on.

CHAIRMAN CHEILEK: Very good. I think we better cut the discussion off at this time and perhaps plan on continuing it at the next symposium. Do you care to schedule it at this time?

I would like to thank the Panel for a very fine set of recommendations. I, for one, will be looking forward to the next symposium to see how much progress we have made, and also what happened to the publication curve [Referring to Figure 2 of Mr. Drees' prepared paper, page 320]. Thank you very much.

SUMMARY

PANEL II

AREAS OF RESEARCH RECOMMENDED IN ROTARY-WING AERODYNAMICS

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Recommendations for research in rotary-wing aerodynamics, which are detailed in the prepared papers (pages 303 through 360) of the members of Panel II, are summarized below. Specific suggestions range from a request for an annotated catalog of existing airfoil section data through a recommendation that novel rotor configurations be developed.

For convenience in summarizing these recommendations, six general categories have been established. It should be noted that no attempt has been made to assign priorities to either the categories or the specific recommendations.

1. AERODYNAMIC CHARACTERISTICS OF AIRFOILS

- Reduce drag through maintenance of laminar flow in the boundary layer
- Define unsteady stall behavior
- Define compressibility effects
- Determine airfoil shape tolerances
- Summarize existing measured airfoil data

2. AIRFOIL CHARACTERISTICS IN ROTOR INDUCED FLOW

- Investigate induced flow field
- Determine effects of rotor wake position
- Define yaw (sweep) effects
- Determine rotating blade boundary layer characteristics

3. ROTOR OPERATING REGIMES

- Determine aerodynamic maneuver loads in nonpropulsive flight modes
- Investigate aerodynamics of stopped and slowly rotating rotors
- Analyze the aerodynamic propulsive capability of rotors in the 200 to 300 knot range
- Study the effects of high advance ratios for various degrees of unloading
- Define the autorotational characteristics of an unloaded rotor

4. INTERFERENCE EFFECTS

- Determine rotor-wing-propeller interference effects for a range of relative loadings and advance ratios
- Determine the effect of a wing on rotor autorotational characteristics
- Alleviate effects of aerodynamic fuselage download
- Define mutual effects of a rotor and an auxiliary thruster
- Develop a better understanding of ground effects
- Improve the predictions of wind tunnel wall effects

5. COMPONENT DRAG

- Reduce hub and pylon drag
- Reduce protuberance drag (e.g., weapons)

6. CONFIGURATION STUDIES

- Perform trade-off studies on aerodynamic efficiences of composite and compound configurations
- Determine effects of modifying rotor load distribution by changes of geometry or by auxiliary blowing (e.g., jet flap in its various forms)

ALPHABETICAL INDEX TO SYMPOSIUM PARTICIPANTS

(Number Refers to Volume in which Formal Presentation Appears)

*Session or Panel Chairman

CROSS-REFERENCE INDEX TO SYMPOSIUM PROCEEDINGS

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